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# THE DIVINE NAMES IN GENESIS

# THE DIVINE NAMES IN GENESIS

BY THE REV.

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# PREFACE

THIS volume requires little more in the way of preface than is said in its introductory pages. It is mainly a reprint of six articles which appeared under the same title in the *Expositor* from April to September, 1913. These articles are here reproduced with a few unimportant changes, and with the addition of a passage (pp. 136–157; also a note on p. 164 f.) written for the *Expositor* but omitted in publication. They were occupied exclusively with questions raised by the first section of Johannes Dahse's *Textkritische Materialien zur Hexateuchfrage*, in which he deals with what he considers the foundation of the documentary theory of the Pentateuch, "the Names of God in Genesis." It seemed to me that that subject was sufficiently distinct and sufficiently important to be treated by itself, apart from the other matters discussed in the same volume. I need hardly say, however, that I had read the whole book, and satisfied myself that it advanced no consideration against the general critical theory which I was not prepared to meet, or which would invalidate any position I had taken up. In a reply to my criticisms in the December issue of the *Expositor* Dahse complains that I had taken no notice of his new hypothesis regarding

the Priestly Code, which he conceives to have a vital bearing on his equally novel explanation of the distribution of divine names. Whether the complaint be reasonable or not, that defect is now supplied in section VII of the present treatise. I have added a chapter (VIII) in reply to Dahse's December article, which I take leave to characterize as a well-meant attempt to "entangle me in my talk." It will be seen that I have found no occasion to modify in any way the opinions set forth in the original articles.

The idea of republishing the articles was first recommended to me by the weighty advice of Dr. Driver, who took a keen interest in the discussion up to the verge of his fatal illness. While I alone am responsible for the views expressed in the following pages, I put them forward with the greater confidence from knowing that they met with the general approval of one whose accurate and comprehensive scholarship and sound judgment are so universally recognized and esteemed. It is with a deep sense of personal loss that I here record for the last time my indebtedness to him, not merely for suggestions that have strengthened my argument at many points, but for the wise and helpful counsel and encouragement which he was always ready to impart to fellow-workers in the field of Old Testament study.

TREVONE, CORNWALL,
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# THE DIVINE NAMES IN GENESIS

THE question which I propose to discuss in the following pages is one of great interest, but at the same time of almost unmanageable complexity. It is matter of general knowledge that for many years back Old Testament scholars have adopted what is known as the documentary theory of the Pentateuch, and that that theory originated in observation of the names for God used in different places of the Pentateuch, and especially in the Book of Genesis. Most readers are also aware that of recent years this aspect of the theory has been subjected to persistent, and sometimes embittered, attack on the lines of textual criticism. We have been told in every accent of assurance, from the cool emphasis of Eerdmans to the superheated invective of Wiener, that the "higher" critics have built their house on a rotten foundation. They have worked with a blind faith in the inerrancy of the Hebrew text, and have been too slothful to examine the evidence for and against the soundness of that text. The assailants on their part have certainly not been slothful. They have striven with might and main to discredit the Hebrew text, and have not been backward in proclaiming their own success. They believe their hour of com-

plete triumph is at hand. Insinuations have not been wanting that nothing but the arrogant and disingenuous perversity of a few individuals, whose scholarly reputation is at stake, keeps an exploded theory in being before a deluded public. But the imposture cannot endure much longer! The linch-pin has been removed from the axle, and the collapse of the cart is momentarily expected.

It must be allowed that under a heavy bombardment the fire of the besieged has been slack. The critics have seemed to say in effect that since the enemy's shells were falling wide there was no need to waste powder merely to add to the noise. Their silence has evidently been misunderstood. It has not been due to a dishonest and cowardly shirking of a discussion in which they knew that they would be worsted, and from which they would emerge as "broken men"! As one of the incriminated persons, I protest that Wiener's war-whoop has not struck one instant's terror into my soul, and that I have even read the temperate arguments of Redpath and Eerdmans with unruffled composure. And I rather think that critics generally have had a better knowledge of the text than their assailants give them credit for. The great quarry in which those who impugn the Hebrew text have been digging of late is the Septuagint. Now every Old Testament scholar is aware that the MSS. of the LXX simply teem with various readings of the divine names, as of many other things besides, and that an immense number of these variants are of no value. Critics had a well-founded suspicion that those on the divine names

in Genesis were no better than the rest. If they have not ceased their work and come down to inspect the supposed foundation of their theory, they have only acted as reasonable men might be expected to act. For one thing, the textual evidence as to the divine names (as I shall show later on) has much less importance than certain writers imagine. In the next place the Hebrew text possesses credentials to which no version, and perhaps the LXX least of all, can pretend. Moreover, the criticism of the LXX is even now not sufficiently advanced to enable us to determine in any scientific manner what the original Greek text was; and until that stage has been approached the mass of variants is merely so much evidence of confusion in its text. It is true that while on the whole the LXX is inferior to the Hebrew it can often be used to correct the Hebrew in virtue of the intrinsic superiority of isolated readings. But in dealing with the divine names this resource fails us, because it is very rarely the case that one name is more suitable to the context than the other. Therefore it is unscientific trifling to cite a number of MSS. which differ from the Hebrew, as if any one of these threw doubt on the accuracy of the Hebrew. So that unless the whole business of criticism was to be suspended until LXX scholars had completed their task, the only practical course to follow was to rely on the general soundness of the Hebrew text, and see whether it led to important results. That that confidence has occasionally been pushed too far I am not concerned to deny, but that in the main

it has been justified by its fruits remains for me an indisputable fact. When it is added that in the attack textual work has often been associated with improbable explanations and arbitrary theories, as in the case of Redpath and Eerdmans, or with hastily improvised scholarship, as in the case of Wiener, there is little to wonder at in the attitude of reserve which upholders of the documentary hypothesis have hitherto mostly observed in regard to this matter.

But there are obvious reasons why an attitude of defensive silence cannot be indefinitely prolonged. We must frankly acknowledge that the trustworthiness of the Hebrew text in its transmission of the divine names calls for more thorough investigation than it has yet received at the hands of critical scholars. Whether the impulse to that investigation comes from one side of the controversy or the other is, or ought to be, a matter of indifference: provided the question is raised in a judicial and scholarly manner, it is right and proper that it should be examined. It may be a regrettable circumstance that the initiative has been left to opponents of the critical position; but they at least need not complain if the advantage of the attack has fallen to them. It is none the less the duty of the critics to put before the public the grounds on which they withhold assent from the conclusions so confidently urged upon them.

The immediate occasion of these remarks is the appearance of a new book by Johannes Dahse,*

* "Textkritische Materialen zur Hexateuchfrage": I. *Die Gottesnamen der Genesis*; Jakob und Israel; *P in Genesis 12–50* (1912).

a German pastor who has already done excellent work in the department of LXX criticism. In many ways the book marks a great advance in the treatment of the question before us. The author is a competent scholar who has devoted many years to the investigation of the subject. He has contributed a number of acute and interesting observations on the minutiae of the text; and has collected and tabulated the textual data of the LXX in a form which, though unfortunately not free from errors and defects, nevertheless represents an approach to completeness which has never been realized before. He has sought to establish the existence of recensions of the LXX which rest on earlier recensions of the Hebrew. A still more important advantage is that he does not confine himself to negative criticism, but brings forward a positive solution of the problem which has at least the charm of novelty. Over against the documentary hypothesis he will set a "pericope-hypothesis," worked out with great ingenuity. Last, but not least, he maintains a tone of uniform respect and courtesy towards his opponents. I do not mean that Dahse is the first on his side to exhibit these qualities, but we have had enough of their opposites to make us feel that we could do with a little more of them.

I wish, then, to take this opportunity to explain and defend the sceptical attitude which I hold as regards this whole movement to undermine the foundation of the documentary theory by destructive criticism of the Hebrew text. Dahse's work has raised many new points, and though I shall

not be able to deal with them all, I shall try to meet the issues new and old impartially and candidly. But I will say at once that I have seen no reason to modify appreciably the opinion I expressed in writing on Genesis a few years ago. I may find occasion as I proceed to reply to some of the strictures which Dahse passes on positions I then took up; but that is quite a subordinate interest. The main issue as between Dahse and me is wrapped up in his acceptance of a challenge which I ventured then to throw out. He writes, "Skinner hat mir auf meinen ARW-Aufsatz erwidert, nur dann sei meine Hypothese von dem Einfluss der Vorlesungspraxis auf den Gebrauch der Gottesnamen bewiesen, wenn sie im einzelnen sich durchführen lasse. Ich denke, im vorstehenden ist das nunmehr zur Genüge geschehen und für Gen. 12–50 dieser Einfluss endgültig nachgewiesen" (p. 97). I will try to show that he has not succeeded.

It may be necessary at the outset to put the reader on his guard against a misleading assumption which underlies much of what is written on the opposite side of this controversy. It is usually asserted, and constantly taken for granted, that the documentary analysis of the Pentateuch depends on the distinctive use of the divine names in different sections to such a degree that if this criterion can be shown to be unreliable the whole edifice crumbles to the ground. That is a very great exaggeration. Dahse ought to know this, for he quotes no fewer than four passages from various writers (one of them friendly to his enterprise) in which the case is stated with

perfect precision and clearness. Yet he sets these aside as "shilly-shally" ("halb 'Ja' halb 'Nein'") evasions; and roundly asserts (p. 121) that "im Grunde genommen auch heute noch die ganze Quellenscheidung von dem Gebrauch der Gottesnamen abhängt"! * There is really no cause for evasion: the issue is very simple and easy of apprehension. It is *true* to say that the use of the divine names was the critical fact first observed (by Astruc in 1753) which furnished a positive clue to the separation of documents in Genesis, and that it is still regarded as a valuable aid to the analysis. It is *untrue* to say that it is the sole criterion, or that apart from it there would be no evidence of diversity of authorship in the Pentateuch at all. A moment's reflection might convince any one that if Astruc's discovery

* Dahse devotes nearly five pages of his book (116 ff.) to a series of extracts from Gunkel's commentary, to show that the analysis still depends on the names for God; and he does me the occasional honour of associating my name with his. There I believe he does a grave injustice to Gunkel, as he certainly does to me. It is an injustice to Gunkel to cite the words which refer to the divine names and omit nearly all the other criteria adduced in connexion with them. As for my own observations, I should hope that any one with eyes in his head will see even from the sentences quoted that I am utterly sceptical of any analysis that depends solely on isolated occurrences of *Yahwe* or *Elohim*. If he had had occasion to read my book through, Dahse would have found that on p. 155 I have ventured to suggest a division of sources which sets aside a universally attested occurrence of Elohim. The paragraph in which he professes to sum up the effect of these citations (p. 121) contains misunderstandings or misrepresentations of the plain meaning of language which are difficult to reconcile with a dispassionate regard for an opponent's position.

had never led to anything beyond itself—if no difference could be observed between documents except their use of the names for God—it would have lost all its interest long ago. Its whole value springs from the fact that almost immediately it led on to the discovery of characteristic differences in the documents—"clearly marked and distinctive linguistic character," "numerous differences in subject matter, and distinguishable varieties of religious and historical points of view" (as Dahse's friendly correspondent puts it). These characteristic features were of course not all perceived at once; but having been worked out by patient and minute research they now afford criteria of authorship sufficiently striking to enable us in many important cases to dispense with the evidence of the divine names. That this is no empty vaunt is capable of experimental proof from two incidents in the history of the problem. (*a*) There was a stage of Pentateuchal criticism when practically only two writers were recognized in the Book of Genesis, an Elohist and a Yahwist. In 1853 Hupfeld showed to the ultimate satisfaction of all critics that there were really two writers using the name *Elohim*, and he succeeded in separating them with a very remarkable approach to finality. This important critical operation was necessarily carried through without assistance from the names for God; and in fact it turned out, as Hupfeld himself perceived, that the general affinities between the two Elohists were not nearly so close as those between one of them and the Yahwist (J). Yet every critic would admit that the achievement ranks with the surest

results of literary analysis. (*b*) After Exodus vi. 2 the divine name ceases to be a criterion of the three sources distinguished in Genesis. One Elohistic document (now called the Priestly Code [P]) regularly uses *Yahwe* henceforward, and the other (E) uses sometimes *Yahwe* and sometimes *Elohim*. But, although this fact increases the difficulty of distinguishing J from E, it does not in the least affect the separation of P from J, which can be performed with as much certainty in Exodus and the following books, without the criterion of the divine names, as in Genesis, where that test is available. It is clear, then, from these examples that in the division of sources which is accepted by the majority of critics the divine names have not the exclusive importance which is attributed to them in the ill-considered utterances of controversial writers on the subject. Similarly the style and character of Deuteronomy stand out clearly from the rest of the Pentateuch, and are entirely independent of the divine name used. In fact the only part of the documentary theory which is largely dependent on the names for God is the separation between J and E.*

* It is noteworthy that the quotations from Gunkel and others referred to in the last note are confined to the JE sections of Genesis—the only sections *within* which the divine names are important for the analysis. On p. 53 Dahse quotes from Driver the remark that if the untrustworthiness of the MT were established "it would leave untouched what is after all the most important element in the critical analysis, viz., the separation of P from JE"; and calmly takes this as an admission that apart from the divine names, J and E could no longer be distinguished! Driver's words certainly imply that the internal analysis of JE would

There the analysis is often uncertain even with the help of the divine names; and of course it would in such cases be still more precarious if that test were proved to be worthless. Now the distinction between J and E is certainly an element of the accepted documentary theory, but it is by no means its most important element. It ought to be clearly recognized that the really vital points in the critical position are the relations to each other of the combined JE, of Deuteronomy, and of the Priestly Code. These relations are established, as we have seen, on grounds which are independent of the use of the divine names by the various writers; and therefore the critical theory would still in all essentials remain intact even if it could be proved that the distribution of the divine names has nothing whatever to do with diversity of documents or of authorship.

After this lengthy explanation the reader will perhaps understand how an adherent of the documentary hypothesis can examine the question of the divine names in Genesis with an easy mind, and without feeling that he is entering on a combat *pro aris et focis*. At the same time it is my purpose to meet Dahse squarely on his chosen field of textual criticism.

I have only to add in the way of introduction that I shall endeavour as far as possible to bring the various matters in dispute within the comprehension of general readers, whose judgment

be "touched" (by the removal of *one* criterion); he has never said or implied that there are no *other* criteria by which an analysis might still be effected.

is otherwise apt to yield to the loudest pretensions and the most assiduous assertion. With this aim in view I have thought it advisable to introduce explanations of technical points which Dahse quite rightly takes for granted as understood by the specialists whom he addresses. Fortunately, in England it is not only specialists who are interested in these discussions; and the more this unprofessional interest can be cultivated the better it will be for the cause of truth. For it remains true that the common sense of mankind, when fairly enlightened, is the "ultimate solvent" of all critical and speculative theories whatsoever.

# I

## EXODUS VI. 2, 3

DAHSE begins with an examination of the text of Exodus vi. 2, 3, quoting from Dr. Carpenter a sentence to the effect that these verses contain the real key to the composition of the Pentateuch. In the Hebrew text they read as follows:

"And *Elohim* spoke to Moses and said, I am *Yahwe*; and I appeared to Abraham, Isaac and Jacob as *El Shaddai*, but by my name *Yahwe* I did not make myself known [or 'I was not known'] to them."

The crucial importance of these words must be apparent to the least critical reader. Three names for the Deity are used: *Elohim*, which is the generic name for God, applied alike to the true God and to heathen deities; *Yahwe*, the proper name of the God of Israel, and in fact *the* name *par excellence* of the true God; and *El Shaddai*, a somewhat rare title of the Deity, whose etymology and historic origin are obscure. And the verses distinctly state (1) that God had revealed Himself to the three patriarchs under the name *El Shaddai*; (2) that He had not disclosed to them His true name *Yahwe*; and (3) that this name is now (for the first

time) made known to Moses. It is evident that the author of these statements cannot have written any passage which implies on the part of the patriarchs a knowledge of the name *Yahwe*, and in particular any passage which records a revelation of God to them under that name. It is conceivable that the writer himself might have used the name *Yahwe* in speaking of God, just as a historian might speak of the childhood of Charlemagne, although no one could have thought of applying that honorific title to him during his early years. But it would not be a very extravagant assumption to expect that the author of Exodus vi. 3 would avoid the anachronism of calling God *Yahwe* before that name was known, and restrict himself to the use of *Elohim* or *El Shaddai*. How far these observations will carry us in the analysis of the Pentateuch we shall see presently.

According to the generally accepted documentary theory of the Pentateuch, the verses Exodus vi. 2, 3 belong to what is called the Priestly Code. As the result of minute and protracted investigations, critics have arrived at an almost perfect consensus of opinion regarding the contents of this document, and it is important here to note that in the course of these investigations the distinctive use of the divine names has come to play a very secondary part. The analytic process has been guided by a number of characteristic features of language and style and thought which make it a comparatively easy thing to detect a fragment of this document even if no divine name occurs at all. If

now we take the Priestly Code as it has emerged from the hands of the critics, we find some remarkable correspondences with our reading of Exodus vi. 2, 3. We find, in the first place, that the name *El Shaddai* actually occurs in the histories of Abraham, Isaac and Jacob: twice in self-revelations of God (xvii. 1, xxxv. 11) and twice in utterances of Isaac (xxviii. 3) and Jacob (xlviii. 3).* It appears nowhere else in this document. We note next that the writer himself, when speaking of God in the third person, up to Exodus vi. 2 invariably uses *Elohim*, save in two instances (xvii. 1, xxi. 1*b*)—by a scribal error, as some think; after Exodus vi. 2 he regularly uses *Yahwe*. Moreover, the first disclosure of the name *El Shaddai* to Abraham (xvii. 1) is in the form "he said to him, I am *El Shaddai*," exactly corresponding to the "he said to him, I am *Yahwe*" of Exodus vi. 2. This careful distinction of three stages of revelation, marked by the names *Elohim*, *El Shaddai*, *Yahwe*, is in strict harmony with the affirmations of Exodus vi. 2, 3: the name *El Shaddai* was revealed to the patriarchs, while the name *Yahwe* was reserved for the crowning revelation to Moses. Whether the critical construction be sound or not, we see that there is ample justification for the statement of Dr. Carpenter that Exodus vi. 2, 3 has proved the "key" to the analysis of the Pentateuch.

But to meet Dahse on his own ground, we must of course start anew from the foundation. We

* Outside of the Code it occurs twice: for details see below.

must try to obliterate from our minds all that we have heard about a Priestly Code, about the sources of the Pentateuch, or about its composite authorship. We must take the bare words of the text by themselves, and inquire whether they be consistent with the supposition that the Pentateuch is a literary unity and the work of a single author. Now we observe (still using our Hebrew Bibles) that the name *Yahwe* is freely used in Genesis and the first five chapters of Exodus. I have already admitted that this fact does not prove that the writers were ignorant of the theory that *Yahwe* was first revealed to Moses. But when we see that there are whole sections of Genesis where *Elohim* alone is used, and others in which *Yahwe* is used, there is surely a presumption that those who held that theory are likely to be the authors of the former and not of the latter. But not to press that point, we look again at our Hebrew text and find the express statement that from the days of Enos men "began to call by the name of *Yahwe*," i.e., to invoke the Deity under that appellation (Gen. iv. 26).* The very same phrase is used of Abraham (xii. 8, xiii. 4, xxi. 33) and of Isaac (xxvi. 25). And that is not all. The name *Yahwe* is constantly found on the lips of the patriarchs (more than forty times) and (to crown all) it is twice used by Yahwe Himself in self-revelations to Abraham (xv. 7) and to Jacob (xxviii. 13).† The

* See NOTE II, p. 269.

† It is a not unimportant confirmation of the critical theory that these two passages are duplicates of two self-disclosures of the Almighty to the same two patriarchs in

inference is irresistible that these passages cannot have been written by the same author as Exodus vi. 2, 3, *if the Hebrew text be correct.* The Pentateuch, therefore, is not a unity; and even if we should never be able to take another step in disentangling its sources, we have to recognize that the axe is already laid at the root of the tree.

We can now understand how Dahse, in his perfectly legitimate attempt to discredit the documentary theory of the Pentateuch, is laid under a necessity to undermine the authority of the Hebrew text. He must either challenge the accuracy of the Hebrew transmission of the divine names throughout Genesis, or make out that the passage in Exodus means something different from what the Hebrew most undoubtedly says. As a matter of fact he essays both; and we have now to examine his treatment of the text of Exodus vi. 2, 3, to which he devotes the first five pages of his book. It is impossible to follow all the windings of his argument, which indeed occasionally leads us up a blind alley, where we have simply to retrace our steps. But I will try to deal fairly and candidly with the really material points on which his whole position seems to hinge. And I do so with sincere respect for the thoroughness of his research and the acuteness of his reasoning.

1. His first point is that the word נודעתי, "I made myself known" or "I was known," is repre-

the PC, the אני יהוה of xv. 7, xxviii. 13 corresponding to the אני אל שדי of xvii. 1, xxxv. 11. See Gunkel, *Genesis*, Ed. 2, p. 342 f.

sented in all texts of the LXX by ἐδήλωσα, which is the equivalent of הודעתי, "I made known." Thus for the statement, "by my name Yahwe I was not known," we obtain the sentence "my name Yahwe I did not make known." Now I propose for the sake of argument to make Dahse a present of this reading. Not that I consider it to have any claim to be preferred to the Hebrew. True, it is supported by the Targum of Onkelos, the Peshitta, the Vulgate, and one Hebrew codex. But there is an almost equal array of external evidence in favour of נודעתי: the Samaritan Pentateuch, the Targum of Jonathan, and all Hebrew codices except one. I believe that an impartial textual critic would say that the external evidence of the MSS. and Versions is pretty evenly divided between the one reading and the other. My preference for the Massoretic reading, however, rests chiefly on the consideration that there is an obvious reason why נודעתי should be rendered by a causative verb, but none at all that I can think of for changing an original הודעתי into נודעתי. The clause שמי נודעתי, although perfectly unexceptionable in syntax, is nevertheless a somewhat subtle Hebrew idiom, and one which a translator might naturally evade without being unfaithful to his text. That the translators actually found הודעתי in their original is certainly possible, but it is not proved; still less is it shown to be a superior reading to the Massoretic נודעתי; for if הודעתי had been the authentic text it is difficult to account for the change to נודעתי. If it be set down as a copyist's slip, we have to ask which is more likely: that the clerical error is

on the side of the overwhelming majority of Hebrew MSS. or on the side of the single codex which reads הודעתי. The agreement of a single codex with one or more versions is not sufficient evidence that the variant reading was once widespread in Hebrew, or that it lies behind the versions in question. There are such things as chance coincidences. But I do not insist on this point, because I am prepared to argue that it makes not the slightest difference to the critical implications of the verse whether we read הודעתי or נודעתי.

2. Nor, again, is it necessary for our immediate purpose to join issue with Dahse on the soundness of the text at the beginning of verse 2, "and *Elohim* spoke to Moses," where he thinks that *Yahwe* stood originally instead of *Elohim*. The former, it appears, is attested by five Greek cursives (**bw np f**),* by the Old Latin version, and by a citation in Justin. It is also the reading of the Samaritan Pentateuch. The Old Latin and Justin are fairly taken as presumptive evidence that the reading is pre-Hexaplaric; i.e., it was found in LXX MSS. before Origen undertook the task of bringing the LXX into closer correspondence with the Hebrew in the monumental work called the Hexapla. It does not follow that it is the older reading, or even that it existed in Hebrew MSS. Many errors had crept into the LXX text before Origen; and for what we know this may be one of them. It seems to me, indeed,

* It will be seen that the MSS. represent three different recensions, but that in the last two cases they are opposed by the majority of the group to which they belong.

that Dahse is much too ready to assume a Hebrew original for any Septuagintal variant which strikes him as significant. On the other hand we must admit that in this case there is one consideration that pleads in favour of יהוה being original. The tendency of the LXX is to substitute ὁ θεός (אלהים) for κύριος (יהוה) rather than *vice versa*; hence, as Dr. Buchanan Gray has remarked, "wherever (ὁ) κύριος appears in LXX it deserves attention as a possible indication of the original text." * Let us grant, then, that the κύριος of the Old Latin and Justin and the יהוה of the Samaritan Pent. in Exodus vi. 2 is a *possible* indication of the original text, and that all the remaining LXX evidence, as well as the Massoretic text, may have to be set aside; how would this affect the use of the passage as a key to the analysis of the Pentateuch? Would it inflict a very deadly blow on the documentary theory if its supporters had to admit that a writer who has avoided the name Yahwe up to this point had anticipated by half a verse the disclosure of the name which he is about to record? I hardly think so; and for that reason I waive the point here, and pass on to others of more importance.†

* The sentence is taken from Dr. Gray's Commentary on Numbers, the Preface to which is dated January, 1903. It is right to point out as against Dahse (Reply, p. 484) that, so far as Dr. Gray is concerned, the statement is in no sense a "concession." It was written before Dahse had appeared on the field of criticism and before (so far as I am aware) there was any acute controversy about the critical value of the divine names.

† Dahse is entitled to make the most of the circumstance

3. We come now to issues of really vital interest. The first is the genuineness of the name *El Shaddai* in Exodus vi. 3. Dahse seeks to prove by a somewhat intricate line of argument that the name is not original, but was introduced into the text by an editor at a comparatively early date (before the time of Origen) and he reaches the same conclusion regarding five out of the six cases where the name appears in Genesis. It is necessary to examine this position very carefully; but the questions raised are extremely complex, and the reader may be prepared for a rather tedious discussion.

Let us look first of all at the actual occurrences of the name. The Hebrew reads *El Shaddai* in Genesis xvii. 1, xxviii. 3, xxxv. 11, xliii. 14, xlviii. 3, xlix. 25. The LXX renders ὁ θεός σου in xvii. 1, xxxv. 11, ὁ θεός μου in xxviii. 3, xliii. 14, xlviii. 3, and ὁ θεὸς ὁ ἐμός in xlix. 25. In Exodus vi. 3, it has θεὸς ὤν αὐτῶν. There are traces of pre-Hexaplaric readings: omission of σοῦ in xvii. 1, xxxv. 11, of μοῦ in xlviii. 3, and of ὤν in Exodus vi. 3; but as these do not materially affect Dahse's final conclusion we shall do him no injustice if we neglect them here.

Now the first thing that strikes us is that the LXX invariably renders *El Shaddai* by ὁ θεός followed by a possessive pronoun in the person

that in Gen. xvii. 1 יהוה stands (by error, as I believe) in an account of the self-revelation of God; and so in xxviii. 13; and to argue that from analogy the same name should be read in Exod. vi. 2. But what of xxxv. 11, where אלהים is all but unanimously supported by the LXX, or xlvi. 2, where no LXX variant is recorded at all?

appropriate to the context. It looks as if the translators had not understood the word שדי, but had the notion that somehow it expressed a closeness of personal relation between the Deity and His worshippers. I confess that I have no satisfying explanation to offer of this strange idea—that שַׁדַּי was equivalent to a possessive pronoun. Eerdmans thinks that the LXX pronounced the name as אל שְׁדִי (*El Shēdī*), "God my demon," and understood *El Shaddai* as the special guardian deity of the individual patriarchs. That explanation is not quite convincing, because it fails to account for the change of the first personal pronoun or adjective to the second or third where the circumstances required it; but I can suggest nothing better. Anyhow, I am in no worse case than Dahse himself; for the difficulty has to be faced in xlix. 25, the only passage in which Dahse allows the name to be genuine. If he can produce an explanation of the ὁ ἐμός in that verse, it will probably suit all the other cases as well. In the meantime I think that we are entitled to hold by the *prima facie* impression which the usage of the LXX makes upon us, viz., that *Shaddai* was a puzzle to them, and that they concealed their embarrassment as best they could.

But let us see how Dahse succeeds (or does not succeed) in eliminating *El Shaddai* from all these passages except one. The writer of Exodus vi. 3, he argues, must have found in Genesis three separate self-revelations of God, to Abraham, Isaac and Jacob; and if he wrote באל שדי he must have found the name in each of these. Now we find such

revelations in the case of Abraham (xvii. 1) and of Jacob (xxxv. 11); but there is none in the history of Isaac. The only passage which the writer of Exodus vi. 3 could have had in view, according to Dahse, is xxvi. 24; and there *El Shaddai* does not occur. Therefore it cannot have stood originally in Exodus vi. 3! Further, in xxvi. 24 God calls Himself "the God of thy father," and similarly to Jacob in xxviii. 13. But the God who is to Isaac and Jacob the God of their father must have named himself to Abraham "thy God"; and this is how we read in the LXX of xvii. 1. Consequently their Hebrew text must have read אלהיך, "thy God," and we must accept this as original! In the same way we must read in xxviii .3, אלהי, "my God," in xxxv. 11 אלהיך (or simply אל), in xliii. 14 אלהי, in xlviii. 3 אלהי (or אל), and in Exodus vi. 3, אלהיהם "their God." The only genuine instance is xlix. 25. A "theological redactor" (*Bearbeiter*) found the name here, and proceeded to insert it in the other passages. Fortunately for Dahse's detective pursuit, he overlooked xxvi. 24.

Such arguments carry no conviction. But since this hypothesis of a theological redactor is an essential part of Dahse's main contention, I will point out some of the difficulties under which it labours.

(1) One would like a better reason than Dahse gives for retaining *El Shaddai* in xlix. 25* while deleting it in all other cases. To be sure the theory would break down unless the name were left in one case; for the supposed theological

* The received Hebrew text has את שדי, but of course I agree with Dahse that אל is the true reading.

redactor must be allowed a little capital to start operations with. But that is not a reason that can be seriously advanced; and Dahse does not advance it. What he says is that the LXX rendering in xlix. 25 is unique. But is it so very unique? Is the difference between μου and ὁ ἐμός so great that a translator who rendered שדי by the one might not also have rendered it by the other? One would have thought that a passage in which *El Shaddai* stands in poetic parallelism with "God of thy father" is the place of all others where we might suspect that it stands for an original אלהי, if one were to indulge such suspicions at all.

(2) The procedure attributed to the redactor is arbitrary and irrational in the extreme. How could it have occurred to any man to manipulate the text by multiplying instances of a most unusual divine name? How does it happen that he confines his operations to the histories of the three patriarchs? Why did he select these particular passages and leave others untouched? Why did he pass over such revelations as xv. 7, xxviii. 13, as well as xxvi. 24? It cannot have been to give an air of reality to the statement in Exodus, for, according to the theory, he was himself responsible for the insertion of the name in Exodus vi. 3. What could have suggested its insertion there? Was it because he took exception to such empty phrases as "my God," "thy God," "their God" on solemn occasions like those before us? That motive would be creditable to his religious instinct, but it is certainly not a probable one. In any case it would not

explain his choice of the particular name *El Shaddai* as a substitute. Dahse may reply that he is not bound to answer such questions as these: he has satisfied himself that the LXX has the superior text, and has suggested an explanation of the Hebrew, and that is enough. But with all respect I submit that the questions are pertinent. Those who do not share Dahse's confidence in the LXX have a right to ask which of two theories is more reasonable: that the strong, clear-cut sense of the Hebrew is the result of redactional action for which it is impossible to find any adequate motive, or that the comparatively weak and pointless LXX reflects the ignorance of Greek translators making the best they could of an unintelligible original.

(3) We have to consider the time at which such a redaction would have been possible. Dahse is at some pains to show that *El Shaddai* must have been found in the Hebrew text in most of the passages in the time of Origen. We may safely assert that it was found in all of them long before then. The Massoretic recension had been fixed by the middle of the second century A.D., and there can be no reasonable doubt that in all essential respects it lay before Origen in the form in which we now have it. But more than that: the Hebrew is supported by the Samaritan Pentateuch. Hence if any such redaction as Dahse supposes ever took place, it must have been at latest in the fourth century B.C., nearly 100 years before the Greek translation was made. I will not deny the possibility that Hebrew MSS. of an older date may

have been in the possession of the Alexandrian translators; but surely the hypothesis that their MSS. had escaped a redaction which must have been carried out at least a century earlier is too incredible to be entertained on such slender grounds as Dahse has produced.

(4) Expressions like "my God," "thy God" are extremely rare in the patriarchal history (xxvii. 20 being the only case at all parallel to those imagined by Dahse): and that should make us cautious in substituting them for a well authenticated Hebrew reading. Still, if there *had* been a redactor on the look out for opportunities of inserting אל שדי there is no apparent reason why he should have passed over xxvii. 20 any more than xxviii. 3, especially if, as Dahse thinks, the original LXX of xxvii. 20 was simply ὁ θεός σου (without κύριος).

(5) It is by no means clear that Exodus vi. 3 presupposes a separate revelation of the divine name to Isaac. It is perhaps enough that Isaac knew the name *El Shaddai*; and *that* we learn from xxviii. 3. At all events xxvi. 24 is a broken reed for Dahse to rely on. We read there certainly of a revelation of God to Isaac; but it is neither as *El Shaddai* nor as "thy God," but as the God of Abraham. Therefore, if "their God" were the right reading in Exodus vi. 3, it must be understood not distributively of each separate patriarch, but collectively, the revelation to Abraham covering the case of Isaac and (if need were) of Jacob also. In precisely the same way we may hold that the Hebrew reading אל שדי is to be taken collectively, i.e., that the disclosure

of the name to Abraham includes its disclosure to Isaac; and we may accept this sense all the more readily because the name is actually used by Isaac (xxviii. 3) in passing on the blessing to Jacob.*

I hold, therefore, that Dahse has entirely failed to dislodge the name *El Shaddai* from Exodus vi. 3.† It stands there, the signature of

* From the standpoint of the critical theory it would be natural to explain the omission of Isaac by the supposition that the section of the Priestly Code in which the revelation to Isaac was recorded had been suppressed in the course of the redaction. I do not myself believe, however, that that is the true explanation. In the older Yahwistic tradition there are two disclosures of the divine name *Yahwe*, one to Abraham (xv. 7) and the other to Jacob (xxviii. 13), but none to Isaac. The authors of the Priestly Code adhered to this tradition of a twofold revelation of the name; only, in accordance with their theory, they changed *Yahwe* into *El Shaddai*. See the footnote on p. 15 above.

† Dahse promises (p. 5) that the reason why *El Shaddai* was inserted in the 6 passages mentioned, and not in xxvi. 24, will be explained in the last part of the volume. He seems to refer to p. 157, where he points out that in xliii. 14 it occurs at the beginning of a new Seder (pericope of the Synagogue lectionary), and adds that the Seder-division shows us why it stands just here: it was inserted here as in xvii. 1, xxviii. 3, xxxv. 11 and xlviii. 3, "after the reading-lessons had been introduced!" Rarely has a point of exclamation concealed such looseness of argument. How in the world do we see that the interpolation is later than the Seder-division? Is it because it never occurs twice in one Seder? Surely that is not very wonderful, seeing there are 37 Sedarim in which it does not occur at all. Moreover, as far as that goes it might just as well have been inserted in xxvi. 24. I suppose that what Dahse would have *liked* to say is that it never occurs except at the beginning of a Seder; but he could not put

an incomplete revelation under which the patriarchs lived. It stands also as the contrast to the name *Yahwe*, which is now for the first time made known to Moses. But here we have to meet another contention of Dahse, directed this time against the very citadel of the critical position, viz., the genuineness of the reading *Yahwe* in verse 3.

4. Dahse thinks it doubtful if the word יהוה stood after שמי in the original text, so that *possibly* we may be right in reading simply "my name I did not make known." The evidence he adduces is of the slightest. The word is omitted only in two unimportant cursive MSS. of the LXX, a MS. of the Ethiopic version (which is derived from the LXX), and in citations of Justin, Philo, Eusebius, Theodoret, and a few later writers. Dahse appears here to be conscious that he stands on weak ground, for he proceeds to strengthen it by urging that the authority of Justin's citation is much better attested in regard to the κύριος at the beginning of verse 2, which we have already discussed. I must leave it to expert students of the LXX to say whether all this is sufficient to prove that the omission

it that way in view of xxviii. 3, where the name stands in the middle. If he means that it is too remarkable to be a mere accident that in 5 cases (including Exod. vi. 3) out of 6 it stands at the beginning, we must remind him that the phrases "my God," etc., which are supposed to have invited the interpolation, must have stood (on his view) in precisely the same places *before* the Sedarim were instituted, and nowhere else (except in xxvii. 20). The coincidence is no more remarkable in the one case than in the other.

of κύριος points to a pre-Hexaplaric text, although I cannot think that a decision in this sense will be very confidently pronounced. In any case it is not the *only* pre-Hexaplaric text, the κύριος being supported by the consensus of all other LXX codices. The question is, which of the two represents the original LXX, and again, whether the original LXX or the Massoretic text (supposing the two to differ) represents the original Hebrew. And these questions can only be decided by considerations based on the meaning of the passage.

Hence it is of importance to note the use which Dahse would make of the shorter reading, supposing it to be established. If, he says, the *Yahwe* be not original here, then Exodus vi. is not a parallel to Exodus iii., but a continuation of it. He alludes to another part of the documentary theory: viz., the recognition of a third document (known as the Elohistic), which records the first revelation of Yahwe to Moses in Exodus iii. 14, 15, and consistently avoids the name up to that point. On that view Exodus iii. 13 f. and vi. 2 f. are parallel accounts of the same incident by two different writers (E and P). Dahse's reading of vi. 3 enables him to repudiate that analysis, and to hold that vi. 3 refers back to and presupposes iii. 13 ff. But what follows? Simply this: that the "name" revealed to Moses, and not revealed to the fathers, *is* Yahwe after all: only, the revelation was not made on this particular occasion but a short time previously. In other words, Dahse will have succeeded in overthrowing one particular point in the docu-

mentary theory, but he leaves intact the key to the position, in the statement that the name Yahwe was first made known to Moses.

5. But in order to appreciate the full force of Dahse's contention, we must take account of another assertion which he makes. He will not admit that the formula אני יהוה at the end of verse 2 is a new self-manifestation of God. That depends entirely on whether or not the name had been revealed before. Critical writers hold that it appears here for the first time in a particular document; Dahse denies this; and until that point is settled it is idle to discuss whether the phrase in the instance before us marks a new disclosure of the divine name. It is at least a very solemn re-affirmation of it. But look at the verse again. Dahse, if I understand him aright, concedes that "my name" *means* Yahwe even if the writer did not expressly say so. Thus we cannot get behind the statement that God did not reveal the name Yahwe to the patriarchs, whereas He *does* reveal it to Moses. The only advantage that Dahse can derive from his two contentions is the opportunity of maintaining that the revelation did not take place in Egypt but a short time previously at Sinai. And that leaves the main critical position untouched.

6. We can now see how utterly irrelevant is the distinction between נודעתי, "I was known," and הודעתי, "I made known." Dahse apparently thinks it important. He remarks in a footnote on page 2, after citing two examples of the use of *Yahwe* in Genesis, "not however in words of God Him-

self, cf. LXX Genesis xv. 7, xxviii. 13." I suspect that he has in mind an ingeniously futile notion of his lively confederate, H. M. Wiener, who has learned from the anthropologists that "many savages" have an intense aversion from uttering their own name, while making no objection to being accosted by it, or even to its being divulged to a stranger by a third party. The Israelites of the Mosaic age being in a "very rudimentary" intellectual condition, we may believe that Moses was capable of attributing this superstitious feeling to his God; and there you have the wide difference between הודעתי and נודעתי in Exodus vi. 3. We need not discuss this solution: it will be time to do that when some evidence is produced of the existence of the superstition in question amongst the Hebrews at any stage of mental development. Here it is enough to say that it does not meet the real difficulty, which is to know how, without a previous revelation, the patriarchs were in a position to "accost" the Deity by His true name. For surely Dahse, as a Christian theologian, knows that in the thought of Old Testament writers a knowledge of the divine name can only be gained through a self-revelation of the Deity. It is neither a human invention, nor discoverable by human guess-work. Therefore if he admits the use of the name *Yahwe* by the patriarchs (and I do not understand him to deny this), he must allow us to postulate such a revelation, even if it were not recorded. And if, as I believe to be the case, his assault on the integrity of Exodus vi. 2 f. has demonstrably failed, the only resource by which he can save

the unity of the Pentateuch is to follow the example of two Catholic writers * whom he mentions, and maintain that in the original text of Genesis i.–Exodus iii. 12, *Yahwe* never occurred at all.

My reply to Dahse, as regards Exodus vi. 2, 3, may be summed up under three heads. I claim to have shown (1) that he has failed to make good his objections to the Hebrew text of the verses; (2) that if we grant some of his positions the evidential value of the passage for the purpose of critical analysis is not greatly affected;

* Hummelauer and Hoberg (see p. 21 f.). Another Catholic writer, Professor Schlögl of Vienna, has arrived at the same conclusion. He has published in the *Expository Times* (September, 1909, p. 563) a "zusammenfassende Statistik" of the results reached by himself and his pupils in Seminar; and winds up with the following *Machtspruch*: "When we consider that the tendency to use יהוה for or along with אלהים was incomparably greater than the contrary, those few passages which support יהוה as against אלהים are of little account. The conclusion is therefore justified that the name יהוה did not originally occur in Genesis i. 1–Exodus iii. 12. It is consequently quite unscientific to determine the analysis of a source by the names of God." His reasoning comes to something like this: in 118 cases where MT has *Yahwe*, "other texts" (no matter what!) have *Elohim* or *Yahwe Elohim*: therefore, in all read *Elohim*. In 30 passages *all* the texts read *Yahwe*: therefore change it to *Elohim*. In 59 places where MT has *Elohim* the "other texts" have *Yahwe* and in 47 *Yahwe Elohim*: therefore, read *Elohim*. "Those texts which have the name אלהים instead of יהוה are less important": nevertheless still read *Elohim*. Could arbitrariness further go? I have no doubt that the work of the Hebrew Seminar at Vienna is very thorough and meritorious; but it is really a little too much to expect independent students to invest its decisions with a Papal infallibility.

and (3) that if we grant *all* his contentions he gains his end only by emptying the words of definite meaning and significance. They would read thus: "And Yahwe spoke to Moses and said, I am Yahwe. and I appeared to Abraham, Isaac and Jacob, being their God; but my name I did not make known to them." So we are left with the following bald and jejune statement as the gist of the communication imparted to Moses on a solemn occasion: that Yahwe had appeared to the three patriarchs but without giving His name! Whether the meaning be that, while the patriarchs knew the name, it was not Yahwe who revealed it; or that, they being ignorant of it, it had been revealed to Moses at an earlier time; or that it is now revealed for the first time; or that the name is something other than Yahwe—something ineffable, which had not been disclosed before and is not disclosed now—we cannot tell. Such is the plight to which we are reduced by a textual criticism which is divorced alike from exegetical intelligence and historical and religious insight.*

* See NOTE III, p. 270.

## II

## THE PERICOPE-HYPOTHESIS

THE main thesis of Dahse's treatise cannot be more succinctly stated than in his own words (p. 99): "The divine names have nothing to do with this or that document, but are *variable elements of the text.*" His most original contribution to the investigation of the subject is an attempt to trace this variation through successive redactions of the text based upon the divisions of the Law in the lectionary of the Synagogue. The general idea that the distribution of the names for God is somehow influenced by the Synagogue reading is, indeed, not new; but so far as I am aware Dahse is the first who has worked it out in elaborate detail, and constructed a theory by which the perplexing phenomena of the present text may be explained. It is this theory which I now proceed to expound and to criticize. Its complexity is such that I almost despair of carrying the attention of the reader with me through the labyrinth of discussion into which we must enter.

Perhaps a short preliminary explanation will be found useful. For the purposes of the Sabbath reading in the Synagogue, the Pentateuch was divided into sections on two different systems.

On one system, which was introduced in the Synagogues of Palestine, the Law was read through in three (or three and a half) years, and was divided into short sections called Sedarim. The other scheme, which seems to have originated in Babylonia, contemplated a reading of the Law in one year; consequently its sections, called Parashas (פרשיות, pl.), are on an average three times as long as the Sedarim. Thus the number of Parashas is 54, and that of the Sedarim is normally 154, although it is variously given as 161, 167, and even 175. In Genesis there are 43 (or 45) Sedarim and 12 Parashas. Now Dahse's theory, very roughly stated, is that the LXX text, as regards the divine names, is regulated by the Seder-division, while the Hebrew is influenced by the Parasha-division. And since the former division is known to be older than the latter, he concludes that the LXX represents an earlier stage of the text than the Hebrew. That, by the way, is a pure assumption. From the fact that the Seder-division is the older, it by no means follows that any problematical influence of that division on the divine names is prior to the fixation of the Massoretic text.

But here we must digress for a little to consider a question which Dahse has not thought it worth while to discuss, although it is surely vital to the argument, viz., the antiquity of the Synagogue lectionaries. On this point we have no certain information. Jewish tradition, which on such matters is utterly unreliable, attributes the system partly to Ezra and partly to Moses. Dahse assumes that the Sedarim were arranged by Ezra

in connexion with the final redaction of the Pentateuch (p. 161); but he has no proof of this apart from his own speculative combinations.* An eminent Jewish scholar, Dr. Büchler, who has discussed the subject with great learning in the *Jewish Quarterly Review*,† arrives at the conclusion that the system was slowly developed under definite historical influences extending over four centuries. The earliest stage was the reading of prescribed portions of the Law at the three annual Feasts, which was introduced, as the result of a dispute with the Samaritans, about 200 B.C. The next was the selection of lessons for four special Sabbaths, and this dates from the time of the victory of the Pharisees over the Sadducees in B.C. 79. As to the completed Sedarim-lectionary, with which we are here concerned, all he ventures to say is that it was in use before the Christian era, and that it was considered ancient by Josephus in his time (c. 100 A.D.). He thinks the Parasha-division may have been introduced in the Synagogues of Babylonia by Rab about 200 A.D. Now these views may be right or they may be wrong; but the fact that they are advanced by a distinguished authority makes it very hazardous to build a hypothesis on the assumption that the Sedarim are of great antiquity. But, further, I think we can with great probability assign a superior limit for their introduction. The Samaritan Pentateuch has a division into sections (קצים) which is entirely different from the Jewish. Is it likely that if

* See pp. 225 ff. below.

† Vol. v. p. 420 ff.

the Triennial Cycle had been known from the time of Ezra the Samaritans would have ignored it and devised an independent system for themselves? We may reasonably infer that the Sedarim are of later origin than the time when the Samaritans took over the Pentateuch from the Jews.

This is not the only inconvenient fact with which Dahse deals in too summary a fashion. He is aware that the division of the Sedarim varies in different MSS.; that, e.g., while his authority, Baer, gives 43 in Genesis, the standard Massoretic authority, Jacob ben-Hayyim, gives 45, and that the latter has sections beginning at xi. 1, xxii. 20, xl. 1, xlix. 27, where the former has none; and, on the other hand, that the former has beginnings at xii. 10, xvii. 1, where the latter has none.* Dahse says this is immaterial to his system (p. v); and indeed I am disposed to agree with him, for his theory seems elastic enough to fit a great many divisions of the text. But a theory to which it makes no difference whether or not a new Seder begins at xii. 10, or whether chaps. xvi. and xvii. form one Seder or two, may surely be suspected of undue laxity of principle.† But let us now resume our exposition.

If Dahse's hypothesis were, as a superficial reader might be apt to imagine, that the LXX and the Hebrew keep to one divine name through-

* On the different divisions which obtained in different Massoretic schools, see Ginsburg, *Introduction*, pp. 33–35. The diversity is much greater than I have stated above.

† As a matter of fact, Dahse makes a great deal of the division between xii. 1–9 and xii. 10–xiii. 18, and also of the fact that xvii. 1 is the commencement of a Seder.

out a Seder and a Parasha respectively, the issue would be simple. It would be easy to test the theory, and if it were found correct it would be all over with the documentary analysis of Genesis in so far as it depends on the use of the divine names. But the system is much more complex than this. (1) The editors had a mixed text to start with, i.e., one in which יהוה and אלהים occurred in irregular alternation. Of course it is this mixed original text that we want to get back to in order to see whether it affords any clue to a division of sources. Dahse's theory bars the way. He assures us that the original text is hopelessly obscured by subsequent editings, *more* hopelessly in the Massoretic text than in the LXX (p. 95). Still it is something to know that there *was* an original mixed text, and, though we can no longer be sure, we may surmise that it had something to do with a diversity of authorship to which so many independent circumstances point. (2) The editors of the Hebrew text underlying the *Septuagint* (who operated with Sedarim) were guided by the following rules: They never (practically) change an *Elohim* into *Yahwe*; but in certain circumstances they change a *Yahwe* into *Elohim*. If they found either name used consistently throughout a Seder, they allowed it to stand. But if a Seder contained both *Yahwe* and *Elohim*, their practice was to let *Yahwe* stand at the beginning or end, and elsewhere to change it to *Elohim*.* (3) The editors of the *Massoretic*

* P. 93: ". . . die Stellen wo יהוה=κύριος *immer* den Anfang und Schluss (resp. ersten und letzten Gottesnamen) eines der alten Sedarim . . . enthalten."

*text* (operating with Parashas) were "influenced" by the Parasha-division to this extent that they replaced "the *Elohims* standing in the middle of the Sedarim by *Yahwe*," but only in "Elohimstellen . . . die mitten zwischen Yahweabschnitten lagen" (p. 94)—whatever that may mean. Whether they are supposed to have worked on the original mixed text, or on the recension already prepared for the LXX, Dahse nowhere expressly informs us, and I have been unable to discover for myself. These appear to be the leading features of the hypothesis, so far as they can be made out from Dahse's not very lucid or consecutive description.

Now before proceeding to test the theory in detail, I think it is not unfair to raise at once the question of its inherent credibility. There are three points to consider:—

1. We have to ask what time can be allowed for these postulated redactions. The Samaritan text agrees with the Massoretic as regards the divine names in all but eight or nine cases,* so that, on Dahse's theory, both the assumed redactions must have been completed in the Synagogues of Palestine before the two texts parted company. We have seen reason to believe that the Seder-division of the Law is much younger than the Samaritan Pentateuch, and therefore the redactions could not even have been commenced until

* According to the text of Walton's Polyglot, Sam. reads אדני for יהוה in Genesis vii. 1, xiv. 22, and xx. 18; יהוה for אדני in vii. 9, xxviii. 4, xxxi. 7, 9, 16; and adds אדני in xxxv. 9. The Sam. reading in vii. 1 is not quite certain. In xiv. 22 the names are a late addition to the text.

a long time after the separation had taken place. And apart from that, is it credible that the LXX translators had got hold of an obsolete Palestinian recension, which must have had a very short lease of life, and made it the basis of their version? That by itself is not absolutely impossible; but it will require pretty clear evidence to establish a theory in the teeth of so many improbabilities.

2. We must have some respect for the psychology of the supposed redactors. We are not at liberty to attribute to them *any* course of action that might bring about the actual result, as if they were dilettante triflers amusing themselves by inventing an elaborate cipher to tax the ingenuity of twentieth-century critics. We must treat them as reasonable human beings, working from intelligible motives for intelligible practical ends. In short, we must be able to see that their *modus operandi* is directed to some useful purpose connected with the public reading of the Law. How does the theory stand this test? To take one example: what could have induced the LXX editors, in a "mixed" Seder, to leave the first and last יהוה standing, and to change the rest? Obviously, thinks Dahse, it was to indicate that *Yahwe* and *Elohim* are one God (p. 97). Granted that the hearers needed that reminder, one fails to see how this device would help them. It would no doubt ensure that on the Sabbath when a "mixed" Seder was read they would hear both names; but when the lesson was an "unmixed" Seder they would be left to their own untutored reason. Why should the suggestion of identity be more necessary in the one case than the other?

Would not consistency rather demand that at least in "unmixed" lessons marked by *Elohim* this name should be once changed to *Yahwe* to avert polytheistic misconceptions? All very trivial finical questions, no doubt! but a textual critic has no right to avoid triviality by vague and unconsidered generalities.

3. It is a most unwarranted assumption that editors of the text would have deliberately altered the divine names from any such artificial motive as that on which Dahse's hypothesis rests. The close agreement of the Jewish and Samaritan Pentateuchs in this respect, as contrasted with their frequent differences in other matters, seems to me a conclusive proof that the most scrupulous attention was paid to the divine names in the transmission of the text. Nor am I prepared to admit that even the LXX editors and copyists ever introduced wanton changes of the names of God. In comparison with the Jews and Samaritans they were somewhat careless, and they may often have substituted what they believed to be the better reading; but that they would have made *systematic* alterations of the kind here supposed I see no reason to believe.*

* Dahse will no doubt appeal to the Elohistic redaction of an entire section of the Psalter, and perhaps also to the regular use of *Yahwe* in the Targum of Onkelos, as evidence of a free handling of the divine names in authoritative Jewish circles at a late period in the history of the text. I deny the force of either analogy. The regular substitution of one divine name for another in writings *not yet canonized* affords no ground for the supposition that at a much earlier time sporadic changes might have been made in the oldest and most highly venerated part of the Canon, the Law. Still less is the levelling tendency of a *translation* (the Targum)

But it will be said that these historical and *a priori* objections must give way to literary evidence, and that if the pericope-hypothesis shows a reasonable correspondence with the facts it must be accepted as proved. That will depend on what we consider to be a reasonable correspondence. The discoverer of a new theory is apt to be satisfied with a degree of approximation to fact which a less interested person finds disappointing; so that unless the correspondence be *exact* (which in this case it is not) there will be room for difference of opinion as to the value of the discovery. We must make the best of it, however; and I will now go on to examine how far Dahse's solution accounts for the distribution of the divine names in the different sections of Genesis.

We may first of all dispose of the very exceptional cases where an אלהים in the Hebrew is represented by (ὁ) κύριος in the LXX. It seems to me that Dahse here somewhat misunderstands the position of his opponents. He constantly argues as if the only possible explanation of the rarity of the instances where κύριος=אלהים were the shrinking of copyists from the use of the sacred tetragrammaton. For my part I have never believed that that is the chief cause of the

an index to what would have been permitted in dealing with the sacred text itself. In any case one fact is not annulled by another. The agreement of the Heb. and Sam. is a critical fact which is explicable only by extreme care in the handling of the names from the time when the two texts diverged; and that is surely a more reliable indication of the feeling of the earliest editors than any preferences which may have asserted themselves in a later age. [See pp. 149 f., 153 below.]

phenomenon in question. I hold that the differences between the LXX and the MT in either direction are due to errors that have crept in during a long series of transcriptions, and that the main reason why ὁ θεός is substituted for κύριος so much more frequently than κύριος for ὁ θεός is simply that ὁ θεός came more readily to the pen of a Greek scribe than the Hebraic κύριος. Be that as it may, there are only three cases in Genesis xii.–l. where Dahse admits that κύριος stands for אלהים, xxi. 2, 6; xxviii. 20 (he might at least have mentioned xix. 29, if not also xvii. 15, xxx. 17, xlviii. 9 [O.L.]. It is important for him to show either that יהוה is the original Hebrew, or that ὁ θεός is the original LXX. I will not here pause to discuss the readings. It is enough to say that as regards xxviii. 20 he seems (pp. 96, 106) to make out a good case for יהוה as the original text; but as regards xxi. 2, 6 his reasoning (pp. 102, 111) appears to me utterly weak and inconclusive.

We come at last to the crucial test, a comparison of Dahse's theory with the facts that lie before us in the two texts, the Massoretic and the Greek. And here my observations are so opposed to Dahse's generalized statements that I find it necessary to visualize them, in order that the reader may see at a glance how the matter stands. In the following synopsis* I register the occurrences of the names יהוה and אלהים (J = יהוה, E = אלהים) for each Seder in Genesis xii.–l., first according to the MT and then according to the LXX. The second line

* Pp. 44–47 below.

gives the readings of the larger Cambridge LXX, except in a few cases where another reading seems better attested: but in a third line I have noted those readings which are expressly claimed by Dahse as original. The material is taken from his own table, save in the few places where I have happened to detect an error. For the present I confine the examination to chaps. xii.–l., because in the first eleven chapters the frequent occurrence of the double name κύριος ὁ θεός in the LXX creates a special and complicated problem.

I do not know whether Dahse will maintain that these statistics bear out his pericope-theory, or whether he will challenge them. If he elects for the latter alternative, there is certainly a whole jungle of problematical restorations of the original LXX in which he may take refuge, and through which it will be difficult for a non-expert critic to follow him. I will deal with some of his ventures in this field in other connexions; in the meantime I will only say that he has no right to make capital of our ignorance by subjective speculation as to what the original LXX *must* have been. His theory must be judged in the light of the textual data which we possess; and behind the readings best established as original no theorist is entitled to go. For it is not a readjustment here and there that will save this theory, but a wholesale reconstruction on a scale which no sane scholar will either attempt or justify.

The plain truth is that between Dahse's observation of the facts and mine there are

| Par. | Seder. | Contains. | Names. | Remarks. |
|---|---|---|---|---|
| III ($12^{1}$–$17^{27}$) | 9 | $12^{1-9}$ | MT: J 6 times<br>LXX: J 6 times | |
| | 10 | $12^{10}$–$13^{18}$ | MT: JJJJJJJ<br>LXX: EJEEEEJ †<br>*E | * ? $13^{4}$ (see Dah. pp. 102, 92 f.) |
| | 11 | Ch. 14 | MT: J<br>LXX: – | $14^{22}$: Sam. E ‡ |
| | 12 | Ch. 15 | MT: JJJJJJJ<br>LXX: JJJEEJE<br>-- ?? | $15^{2}$ (p. 13) $15^{4}$ (pp. 102, 105) $15^{8}$ (p. 12 f.) $15^{18}$ (pp. 41, 109 f.) |
| | 13 | Ch. 16 | MT: JJJ-JJJJJ<br>LXX: JEJJJJJJJ<br>E | $16^{11}$ (p. 110, cf. pp. 22, 32) |
| | 14 | Ch. 17 | MT: JEEEEEEE<br>LXX: JEEEEEEE | $17^{7, 8}$ omitted |
| IV ($18^{1}$–$22^{24}$) | 15 | Ch. 18 | MT: JJJJJJJJJ<br>LXX: EJEJJJJJJJ<br>J J | $18^{1, 14}$ (pp. 92, 102) |
| | 16 | Ch. 19 | MT: JJJJJJEE-<br>LXX: JJJJJJJEJ<br>E E | $19^{13}$ (? p. 110) $19^{29a}$ (p. 111) |

| | | | | |
|---|---|---|---|---|
| | 17 | Ch. 20 | MT: EEEEEEEJ*<br>LXX: EEEEEEEJ | * $20^{18}$: Sam. E |
| | 18 | Ch. 21 | MT: JJEEEEEEEEEEEJ<br>LXX: JJJ EJ EEEEEEEEEJ<br>JE | $21^{4,6}$ (? see pp. 102, 111 f.) |
| | 19 | Ch. 22, 23 | MT: EEEEJEJJJJ*E<br>LXX: EEEEJEJJJJ E § | * Par. IV. ends here ($22^{16}$) |
| V ($23^{1}$–$25^{18}$) | 20 | $24^{1-41}$ | MT: JJJJJJJJJJJ<br>LXX: JJJJJJJJJJJ<br>E | $24^{3,7,12,27}$ & $^{40}$ omitted in D.'s table<br>$24^{40}$ Dah. ὁ θς μου ? (pp. 13, 103) |
| | 21 | $24^{42-67}$ | MT: }<br>LXX: } J 8 times | $27^{42}$, $^{48b}$ omitted by Dahse |
| | 22 | $25^{1-18}$ | MT: }<br>LXX: } E once | |
| VI ($25^{19}$–$28^{9}$) | 23 | $25^{19}$–$26^{34}$ | MT: JJJJJJJJJJJ<br>LXX: JEJJJJJJJJJ<br>E | $26^{28}$ (Dah. ? p. 112) |
| | 24 | $27^{1-27}$ | MT: }<br>LXX: } J 3 times ‖ | |

† One might have accepted J for the first E ($12^{17}$), with Boh. OL, etc., but Dahse (p. 40) argues strongly for E.
‡ I agree with Dahse (p. 11) that the name is interpolated both in MT and Sam.
§ J only with "angel," "oracle," of Yahwe, and in the phrase "Yahwe-Yireh," where Dahse says (wrongly) it could not be altered.
‖ Dahse (p. 103) seems to say that the middle name ($27^{20}$) was originally ὁ θς σου.

| Par. | Seder. | Contains. | Names. | Remarks. |
|---|---|---|---|---|
| | 25 | $27^{28}$–$28^{9}$ | MT: E twice<br>LXX: E twice | |
| VII ($28^{10}$–$32^{3}$) | 26 | $28^{10}$–$29^{30}$ | MT: EJJJEE*EJE †<br>LXX: EJ–JEJJE | $28^{13b}$ added<br>* $28^{20}$. See p. 42 above |
| | 27 | $29^{31}$–$30^{21}$ | MT: JJJJEEEEEE<br>LXX: JJJJEEEEEE | |
| | 28 | $30^{22}$–$31^{2}$ | MT: EEEJJJ<br>LXX: EEEEEJ | * $30^{24}$. See p. 54 f. below |
| | 29 | $31^{3}$–$32^{3}$ | MT: JEEEEEEE–JEEE<br>LXX: JEEEEEEEEE–EE | $31^{5, 29}$ omitted ‡ |
| VIII ($32^{4}$–$36^{43}$) | 30 | $32^{4}$–$33^{17}$ | MT: JEEEEE<br>LXX: JEEEEE | $32^{10}$ added |
| | 31 | $33^{18}$–$35^{8}$ | MT: E 3 times<br>LXX: E 3 times | |
| | 32 | $35^{9}$–$36^{43}$ | MT: E–EEEE<br>LXX: EEEEEE | |
| IX (ch. 37–40) | 33 | Ch. 37 | No divine names | |
| | 34 | Ch. 38 | MT: JJJ<br>LXX: JEE | |

| | | | | |
|---|---|---|---|---|
| | 35 | Ch. 39, 40 | MT: JJJJJEJJJE<br>LXX: JJJJJEJJJE | |
| X ($41^{1}$–$44^{17}$) | 36 | $41^{1-37}$ | MT: } E 5 times<br>LXX: } | |
| | 37 | $41^{38}$–$42^{17}$ | MT: } E 4 times<br>LXX: } | |
| | 38 | $42^{18}$–$43^{13}$ | MT: } E twice<br>LXX: } | |
| | 39 | $43^{14}$–$44^{17}$ | MT: } E twice<br>LXX: } | |
| XI ($44^{18}$–$47^{27}$) | 40 | $44^{18}$–$46^{27}$ | MT: } E 5 times<br>LXX: } | $46^{1b}$ omitted |
| | 41 | $46^{28}$–$47^{31}$ | No divine names | |
| XII ($47^{28}$–$50^{26}$) | 42 | Ch. 48 | MT: } EEEEEE–<br>LXX: } EEEEEEE | |
| | 43 | Ch. 49, 50 | MT: JEEE–E<br>LXX: JEEEEE | $50^{17}$ omitted |

† E only with "angels" or "house" of God.
‡ On Par. VII. see the tables on pp. 55, 56 below.

irreconcilable and sometimes unaccountable discrepancies. A good many of his generalizations appear to be simply loose and inaccurate. The number of "mixed" Sedarim is not 9 but 18; "mindestens *ein* יהוה = *κύριος*" (p. 92) being enough to constitute a mixed section. It is not true (p. 93) of Seder 26 that elsewhere than xxviii. 13*a*, 16 we have אלהים = *ὁ θεός*, for there are two Js (xxviii. 20, 21) for the originality of which in the LXX Dahse has expressly argued (see above). The statement on p. 94 that Par. VI. has "at the beginning purely J passages, and at the end three more," if true, would be fatal to the theory, and is contradicted on the next page; as a matter of fact the Parasha ends with two Es. In S. 16 it is not only the last, but the last two, names that are E in the MT. Ss. 17, 18, 19, even apart from "specially motived passages," were not purely Elohistic in the LXX (see J in xxi. 1*b*, xxi. 2, xxi. 4?, xxi. 6?); and even if they had been they would have been none the less "mixed" by the presence of "motived" Js, and there would have been nothing to prevent MT from regularly changing E to J. S. 10 has two Js (xiii. 4, 18), not "only one," as stated on p. 95. S. 23 is Yahwistic (p. 95) in MT, but not entirely so in the *older* (?) LXX form (xxv. 21*b*). S. 35 contains two Es; therefore is not Yahwistic (*ibid.*).—Other statements are justified only by operations on the text which seem to me doubtful and arbitrary. S. 12 is brought under the theory (p. 92) by no fewer than four changes of the text (xv. 2, 4, 8, 18), all precarious, and the last seemingly in opposition to what Dahse has himself said on

p. 41. Similarly S. 13 is manipulated not only by the change of J to E in xvi. 11 (for which as the earliest *Septuagint* reading there is a good deal to be said), but also by *neglecting* (nicht angeführt!) "fünfmal ἄγγελος κυρίου" (p. 92), for which there is no real justification (see below). Chap. xviii. (S. 15) is excluded from the mixed Sedarim by twice changing E to J, on very weak evidence, amounting in the case of xviii. 1, even in Dahse's estimate, only to a "vielleicht" or a "wohl." Pars. VIII and XII are classed as Elohistic (p. 94) by explaining away the two Js (xxxii. 9 (10), xlix. 18) through the rule that "sacrifices, prayers and praises are offered only to Yahwe, not to Elohim" (p. 96), which again is an unreal restriction (see below). Indeed, the variety of motives assigned for the retention of J by the LXX in particular cases is such as to discount heavily the value of a theory which requires to appeal to them all. And lastly it is an absolute *non sequitur* to argue (p. 93) that because there are "mixed" sections in the MT as well as in the LXX, therefore *all* the mixed Sedarim of the LXX must have been mixed in the Hebrew basis of that version. I do not profess to know all that Dahse may have had in his mind in writing these pages (92–95); but taking the statements as they stand I find them utterly untrustworthy and misleading. Probably few will take the trouble to check them in detail as I have done; but having done so I repeat that to the best of my judgment the facts are as I have given them above, and at any rate not as stated by Dahse. And I might fairly

decline to debate a question on fundamental data which I conceive to be wrongly reported.

It is needless, then, to discuss minutely whether Dahse's theory fits the facts tabulated above; it manifestly does not. But I will point out one or two things. Taking first the recension supposed to lie behind the LXX, in the 14 "unmixed" Sedarim the rule is observed that the LXX has made no change on the original, and so far the theory may be said to be vindicated. Yes, but only on the assumption that here the original text has been preserved by the Hebrew; otherwise we cannot tell what havoc the LXX may have made of sections originally mixed. Again, in the "mixed" Sedarim, it is true that there are only two (19 and 26) which do not either begin or end with J, and of these two it may be said that the first or last J of the original has been retained. I will not absolutely deny that there may be evidence of design here (though I greatly doubt it); but even if it be so it is quite as explicable on the supposition that the LXX is dependent on the MT as on the reverse assumption. I fear this is the only triumph that Dahse can claim for his hypothesis. In all other respects it is plain as day, from the synopsis above, that the treatment of the mixed Sedarim is governed by no principle whatever, unless it be the negative principle of making as few mistakes as possible.

Coming next to the alleged Massoretic recension, we find it encumbered with still greater difficulties. What is conceived to have taken place is a Yahwistic redaction, confined to mixed Sedarim, and applied to these only under peculiar

conditions. It is of course possible to represent that the uniform use of J in six out of the eighteen originally mixed Sedarim (Nos. 10, 12, 13, 15, 23, 34) is due to an operation of this kind; but what of the remaining twelve? Dahse tells us that we are not to look for the alteration except in "Elohimstellen die mitten zwischen Yahweabschnitten lagen" (p. 94). The language is studiously ambiguous. What is a *Stelle?* what is an *Abschnitt?* If *Abschnitten* means Sedarim, the absence of the redaction in the majority of mixed Sedarim would no doubt be explained; but then the operation ought not to have been performed in any one of the six just enumerated. If, on the other hand, *Yahweabschnitten* are sections beginning or ending with an isolated J, the conversion of Nos. 10, 12 and 13 (*not* 15, 23 or 34) would be accounted for, but its absence in other instances (e.g. Nos, 14, 17, 29) becomes inexplicable. Again, if *Elohimstellen* means (as apparently it must) individual occurrences of E within the Seder, the rule will be found to be frequently violated on both sides (Nos. 15, 16, 18, 19, 26, 28, 29, 35); and it rarely makes any difference to the working of the theory what position the names occupy in the Seder, or the Seder in the Parasha. It would seem, in fact, that the Parasha-division could only affect the treatment of the opening and closing Sedarim of the Parasha; of these it could never be said that they stand "mitten zwischen" J-sections. Now that consideration would have prevented the redaction in S. 34; and there are only three other cases (Nos. 14, 29, 43) in which it could have had any influence on such an editorial process as is

here imagined. It is time to ask whether it be really conceivable that any man or body of men should have been governed by the whimsical notions attributed to the Massoretic editors. We could understand a systematic alteration of E to J throughout the Pentateuch; we could even understand such an operation being restricted to mixed Sedarim; but a Yahwistic redaction which refused to touch a mixed section unless it was flanked on both sides by the Tetragrammaton is too remote from the normal practical working of the human mind to be received as a credible explanation of the distribution of the divine names in the Hebrew text, even if it could show a much closer correspondence with the facts than is actually the case. I submit then that no case has been made out for a Yahwistic redaction of the basis of the LXX by Hebrew editors governed by a regard to the Parashas. If there had been a redaction at all, the facts would be much more naturally explained by a tendency to assimilate isolated occurrences of E to the Js on either side of them, than by the complex system elaborated by Dahse. And finally one would like to know why the MT is to be accepted as having preserved the original in the "unmixed" Sedarim, and to be regarded as secondary in the "mixed." Does not this amount to assuming that it is to be trusted when it tells in favour of the hypothesis, and discredited when it makes against it? *

* Even Dahse's own theory, untenable though it is shown to be, works out in a manner eminently favourable to the MT. For in the first place it involves the admission, as we have seen, that in all unmixed Sedarim the MT has

We have not by any means exhausted the list of vaguely arbitrary statements for which Dahse makes himself responsible. I propose to follow him point by point through his analysis of Parasha VII (p. 95 ff.), which seems to have caused him more difficulty than any other in chaps. xii.–l. It extends from ch. xxviii. 10 to xxxii. 3, and includes Sedarim 26–29. He commences with what seems the irrelevant remark that no one has yet noticed how this Parasha begins and ends "artificially" with the "angels of God" (xxviii. 12; xxxii. 1). This statement is not quite correct. The last phrase is not in xxxii. 1, but in xxxii. 3, and it is not מלאכי אלהים but מחנה א׳. Wherein the artificiality of the commencement and ending consists, and how the theory is affected by it, does not appear.—In xxviii. 13 he rejects the second J (with the LXX) as an interpolation in the Hebrew text. It makes no difference to the argument whether it be rejected or retained. But it is read not only by Hexaplaric MSS. of the LXX, and by the Sahidic and Ethiopic versions, but also (in place of ὁ θεός) by the Old Latin; so that it has a good claim to be regarded as the original reading of the LXX. We cannot, in view of xxvi. 24, say that the sense demands it; but at least the sense is better with it than without.—In xxviii. 20 I have already admitted the force

preserved the original names. Further it implies that in mixed sections every J of the LXX must have stood in the original text, so that where MT and LXX agree in reading J, the MT is again true to the original. These two maxims between them account for about 126 names out of 216. Why should we suspect the soundness of the MT in the remaining 90 cases?

of his contention that J is the original reading (LXX, κύριος ὁ θεός or κύριος).—We come next to xxix. 31, 32, 33, 35. Dahse allows that in all four instances J is the oldest attested LXX reading (as MT), but says he has already shown that in the first three אלהים or אל is the original. Of *vv.* 31 and 33 I can discover no previous discussion, and I see no reason for going behind the common tradition of MT and LXX. On *v.* 32 he has argued that the real form of the name of Jacob's eldest son Reuben (ראובל) "proves that יהוה cannot be original." I hope to deal with that type of argument in another connection, and will only say here that it rests on a complete misconception. It is true, however, that the Peshitta here reads *Elohim*, and to that no answer can be made except that the Peshitta is much younger than the original LXX, and that a reading of that version unsupported by Greek evidence is no sufficient reason for questioning the soundness of the MT.* In *v.* 35 he allows that J is the true text, but on the inadequate and erroneous ground that it speaks of the "praise" of Yahwe, and that "one offers sacrifice, prayer and praise only to Yahwe, not to Elohim" (but see xx. 17, cf. xxii. 8, xxvii. 28).—Again, we have differences in xxx. 24, 27, 30. In *v.* 24 the textual evidence for E (against MT) is stronger than usual (LXX, Aquila, Symmachus, Peshitta); on the other hand *all* Hebrew MSS. and Sam. have J, so that the

* On p. 27 we find the statement that xxii. 11, 15 are the only cases where a J of MT, rendered by E in Pesh., is translated by κύριος in the LXX. Dahse must have been nodding here.

external evidence is by no means decisive against MT. We will leave it at that in the meantime, and return to the passage in another connexion (p. 185 f). In *v.* 27 the LXX is supported by the Peshitta alone; but Dahse adds the purely subjective consideration that Laban would not be likely to speak of the blessing of Yahwe! Why not? He does that very thing in xxiv. 31, where there is no dispute as to the text, and where (the Seder being "unmixed") J must, on Dahse's own theory, be regarded as original. In *v.* 30 he accepts J as original.—Lastly, on xxxi. 3–xxxii. 3 he declines to discuss xxxi. 49 because of the notorious corruptions of the text.—He adds the general remark that after "name" and "angel," Yahwe is always represented in LXX by κυρίου. The former statement is true, but has no bearing on Par. VII, where the expression does not occur. The latter is incorrect (see Num. xxii. 22–35 *pass.*), and in any case it is clear from Gen. vi. 2, xxi. 17, xxxi. 11 that the LXX cannot have had any aversion to substituting E for J in this connexion. But let us adopt all these suggested amendments, and see how far the result bears out Dahse's theory. We have to distinguish three stages of the text: the original Hebrew; the original LXX (which proves to be almost identical with our present LXX); and the Massoretic text. We get the following scheme:—

| | | |
|---|---|---|
| Seder 26 (xxviii. 10–xxix. 30) | Orig. | EJ-J E JJE |
| | LXX | EJ-J E JJE |
| | MT | EJJJEEJE |
| 27 (xxix. 31–xxx. 21) | Orig. | EEEJEEEEEE |
| | LXX | J J JJEEEEEE |
| | MT | JJJJEEEEEE |

| | | |
|---|---|---|
| Seder 28 (xxx. 22.–xxxi. 2) | Orig. | EEEEEJ |
| | LXX | EEEEEJ |
| | MT | EEEJJJ |
| 29 (xxxi. 3–xxxii. 3) | Orig. | JEE?EEEEE??EE |
| | LXX | JEEJEEEEEE?EE |
| | MT | JEEEEEEE-JEEE |

Compare this with Dahse's summary (p. 96) :—

"Seder 26 is Elohistic, only the 1st (and connected therewith the 3rd) name is J ;
27 Beginning (xxix. 31 ff.) Yahwistic, then Elohistic ;
,, 28 Elohistic, the last name (xxx. 30) Yahwistic ;
,, 29 Beginning (xxxi. 3) Yahwistic, then Elohistic."

It would seem that Dahse's generalizations are as wide of the mark as ever, and that after all these adjustments of the text he has come no nearer to a proof of his hypothesis. We note in particular (1) that the MT exhibits the tendency to substitute J for E only in three passages at most (xxx. 24, 27, xxxi. 49), while in two (xxviii. 20, xxxi. 11) it changes J to E, and in one (xxviii. 13*b*) it supplies J for a blank in the LXX. (2) That the LXX, in violation of its alleged principle, has three times changed an original E into J (xxix. 31, 32, 33). (3) That the characterization of a Seder as "anfangs jahwistisch, dann elohistisch" is merely a device to save the theory by breaking up a mixed Seder into two unmixed sections. It holds good of S. 27 only *after* the LXX redaction, and therefore cannot be appealed to in explanation of the perfectly arbitrary treatment of the divine names

in the LXX of this section. Further comment is superfluous.

I refrain, for the reason already given, from following Dahse through his discussion of the first eleven chapters. It is besides quite unnecessary to do so; for if the theory breaks down (as I believe I have proved that it does) as regards chaps. xii.–l., it fails entirely. I will now ask the reader to dismiss it from his mind and to look once more at the tables given above to see what light they shed on the relations between the LXX and the Massoretic text. It will be found that in 23 out of the 35 Sedarim there is perfect agreement between the two texts; that in 6 there is only one divergence; in four there are 2; and only in two are there so many as 3 and 5 respectively. In all, the divergences number 22 if we exclude cases where a name in one text stands for a blank in the other, or 30 if we include such cases. The total number of occurrences of Yahwe and Elohim in these 39 chapters is 216 in one text and 219 in the other. Here I venture to reaffirm the opinion expressed by me in the *International Critical Commentary on Genesis* (p. xxxv), that that proportion of differences (from one-tenth to one-seventh of the whole) is not so great as to invalidate any critical conclusions properly deduced from the Massoretic text by itself; and further, that the variations are quite adequately explained as accidental aberrations of the LXX, usually in the substitution of ὁ θεός for κύριος, but occasionally in the opposite direction. Let us only conceive (what the solid agreement of

the Hebrew and Samaritan—differing, it will be remembered, only in some eight or nine cases—fully justifies us in assuming) that the MT has preserved the original names with substantial fidelity, and that the LXX is dependent upon it,* and I think that any one not obsessed by a predilection for fine-spun theories and circuitous solutions will perceive that the facts are sufficiently accounted for in this simple way, as they certainly are not by the arbitrary and unintelligible pericope-hypothesis with which this chapter has dealt. It is really carrying a prejudice in favour of the LXX too far to throw the whole textual tradition into the melting-pot, and then to bring out "this calf." I am not now discussing the merits of the documentary theory; my concern is with the Massoretic text. But one remark may be made: whatever may be urged against the documentary theory of the Pentateuch, it cannot be said that on textual grounds it is demonstrably false. I believe I have shown that the pericope-theory of Dahse may be so characterized.

* But see p. 240.

## III

# RECENSIONS OF THE SEPTUAGINT

IN support of his contention that the divine names are a variable element in the textual tradition, Dahse naturally attaches great importance to various recensions of the Greek and Hebrew text which he claims to have discovered, and in which he thinks the names were deliberately altered under the influence of certain recognizable tendencies. Two such recensions we have already had before us: one the assumed Hebrew basis of the LXX, whose existence I have shown to be highly problematical,* and the other the Massoretic text itself. To these he now adds two more, which he identifies first of all in the *Greek* text of two groups of MSS. of the LXX. If he had stopped short at this point it would hardly have been necessary to examine his argument very minutely. But he endeavours to prove that each of these groups "goes back" to a recension of the *Hebrew* text, which may have an authority equal to, or even greater than, the Massoretic recension; and that is a position which evidently requires very careful consideration. In order to put the reader abreast of

* See pp. 50 and 239.

the discussion, I will again commence with some explanatory observations.

1. The word "recension," as used by Dahse, is somewhat ambiguous. In its strict sense it denotes a text established by a systematic revision according to certain critical principles consciously adopted and applied by the editor. Three such recensions of the LXX are known to have been produced in the end of the third and beginning of the fourth century, by Origen, Lucian and Hesychius. Of these the most important and the best understood is that of Origen (the Hexapla). Its character, and the critical lines on which its author proceeded, are sufficiently known from statements of Origen himself, of Jerome and of other patristic writers; and its text is preserved in a number of codices which can be recognized as Hexaplaric by unmistakable external indications. As to the Lucianic and Hesychian recensions there is no reliable tradition beyond the bare facts that they existed, and that at one time they circulated in specified geographical areas. Their text has been lost sight of in the general stream of MS. transmission, and can only be recovered by investigations which are amongst the most delicate and precarious processes of LXX criticism; while the principles that guided their editors are matter for conjecture based on the characteristics of the text thus provisionally ascertained. It is true that some progress has been made in the identification of a certain type of MS. text as Lucianic for a limited number of Old Testament books; but as regards the Hesychian recension only the most tentative steps have

as yet been taken towards the recognition and characterization of its text. Now the recensions to which Dahse here introduces us stand on an entirely different footing. They are *hypothetical* recensions, about which we have no historical information, their existence being merely inferred from the typical textual features observed in particular groups of Greek MSS. No exception need be taken to the use of the term "recension" for a typical text of this kind, provided the problematical character of the revision be clearly kept in view. It must be understood that the discovery of a family likeness in a MS. group does not warrant the inference that we have to do with a recension of the same kind as, say, that of Origen. All that we are entitled to conclude is that the MSS. in question have transmitted the peculiarities of some earlier single codex (called the "archetype" of the group) which may itself have perished. Whether the archetype embodied a deliberate revision of the text, or whether its distinctive readings were merely accidental, is a separate question, which can only be answered, if it can be answered at all, by a demonstration that the text has been treated in accordance with definite canons, implying a conscious purpose of revision. That demonstration, as regards the divine names, Dahse of course attempts to give; but it is clear that he has failed to grasp the significance of the distinction which I have just pointed out. In previous publications * he has sought to identify his two

* *Zeitschrift für die alttest. Wissenschaft*, 1908, pp. 18 ff., 164.

recensions, **egj** and **fir** (see below), with those respectively of Hesychius and Lucian; and he still holds to this opinion * in spite of weighty arguments to the contrary advanced by Hautsch † and others. It is an arguable position. But there is a curious argument on p. 153 (cf. p. 113) of the work before us which shows how little he is prepared to realize the possibility of a wide difference between his recensions and the three great historical recensions of which we have knowledge. It had been urged against his identifications that **fir** is more likely to represent the Hesychian recension than the Lucianic. To this Dahse replies pertinently enough that the Armenian version, which cannot be supposed to have any connexion with Hesychius, has frequent agreements with **fir**. The instructive thing, however, is that he regards this as a confirmation of his view that **fir** is Lucianic. He is blind to the possibility that it may be something different from both, and much less important than either. When a scholar like Dahse deals with the affiliation of LXX MSS. his opinion is that of an expert, and it might be presumptuous for me to question it. Nevertheless it is the truth that, while his grouping of the MSS. has been accepted by other workers in the same field, his identifications of the groups with the historical recensions have met with no support. Professor G. F. Moore, of Andover, who speaks on this subject with an

* *Ibid.*, 1910, p. 281 ff.

† *Mitteilungen des Sept.-Unternehmens.* I, *Der Lukiantext des Oktateuch*, p. 4 f. Comp. Moore, *American Journal of Sem. Literature*, October, 1912, p. 37 ff.

authority second to that of no living scholar, says in the article referred to above that Dahse "has attempted a classification of the codices in Genesis on a very slender basis, and the identification of his groups on a still slenderer one." *

2. In the second place, it is obvious that the establishment of internal Septuagintal recensions, of however comprehensive a character, does not bring us any nearer a proof of the variability of the divine names in the general transmission of the text. It may prove that certain editors of the LXX manipulated the names with great freedom; but that only tends to weaken confidence in the LXX text as a whole, without affecting the stability of the Hebrew tradition which has hitherto been all but universally accepted by commentators and critics of all schools. It is therefore essential to Dahse's argument to show that behind the Greek recensions postulated by him there lie corresponding recensions of the Hebrew text, in which the divine names were already handled with the same freedom and on the same principles as are revealed by the families of Greek MSS. which are supposed to reflect

* On this quotation Dahse remarks (Reply, p. 493) that Moore "did not consider that the value and the grouping of the MSS. in Genesis is totally different from that in Judges." I presume that here "did not consider that" means "has not taken into consideration the fact that." That is for Professor Moore to say; but I fail to see how the retort meets the point of his criticism, which is that Dahse has built his conclusions on a too narrow foundation. On the whole of the above paragraph, see below, p. 246 f.

their characteristics. That step also Dahse takes with full assurance. But it is a step on which a judgment may be formed by any one with a competent knowledge of the textual history of the Old Testament, even if he lack the technical training acquired in the minute comparison of LXX MSS.

These, then, are the two points on which attention must be mainly concentrated in what now follows: (1) We must inquire whether there is sufficient evidence that the hypothetical Greek recensions observe recognizable principles in their treatment of the divine names; and (2) we must examine very carefully the reasons assigned for postulating a Hebrew recension behind the Greek. We approach these questions with an open mind, though perhaps with more circumspection than Dahse thinks called for in the circumstances.

But before coming to that, we must look at a very valuable chapter of the book, in which Dahse discusses the influence on the divine names of Origen's Hexapla—a recension about which, as we have seen, there is nothing hypothetical, but one whose importance for the study of the LXX text can hardly be overrated.

### 1. *The Hexapla of Origen.*

The importance of the Hexapla depends mainly on two facts. In the first place, its influence on the current text of the LXX has been very pervasive. All our extant Greek MSS. are of later date than the time of Origen; and there are few of them, if any, that have wholly escaped

the impress of his recension. Some of the most important codices are distinctly Hexaplaric, and most others, even when their fundamental text is different from the Hexapla, exhibit traces of its peculiar readings. But secondly, it is known that the aim and tendency of Origen's critical work was to assimilate the Greek text to the Massoretic. He did not, indeed, wish to lower the authority of the LXX, which was the accepted canon of the Christian Church in his time; but he sought to indicate the "Hebrew verity" in a way that would be intelligible to a student of his recension. Accordingly, where the LXX differed from the Hebrew he did not venture as a rule on a simple alteration of the Greek; but he gained his end by the use of two critical signs: one (the *obelus* ÷) to mark a word or phrase in the LXX which was not in the Hebrew, and the other (the *asterisk* *) to signify an addition made by himself to bring it into harmony with the Hebrew. When the LXX differed from the Hebrew, not by a simple *plus* or *minus*, but by having a variant text, Origen did not follow any consistent rule, but *sometimes* he used both asterisk and obelus to show that one phrase was to be deleted and the other substituted for it: that is, if one wished to read according to the Hebrew. Thus, to take a simple illustration from the divine names: if Origen found in the LXX ὁ θεός where the Hebrew had κύριος he would obelise ὁ θεός and insert κύριος with the asterisk, thus: * κύριος ⸜÷ ὁ θεός ⸜,* showing at

* The sign ⸜ (*metobelus*) marks the end of the passage governed by the previous sign.

a glance what the exact reading of each text was. Now there is a large number of MSS. which Dahse happily designates "crypto-hexaplaric," in which the text of the Hexapla is preserved, but the signs are omitted: hence the reading * κύριος ÷ ὁ θεός appears in them as the compound name κύριος ὁ θεός. And that is only a particular example of a process of accommodation which has affected the transmission of the LXX text to an indefinite extent; and through the far-spread influence of the Hexapla has introduced into the MSS. a degree of conformity to the Hebrew which has greatly obscured the original character of that version.* There is thus a certain danger that owing to the influence of the Hexapla the ordinary text of the LXX may exhibit, in its use of the divine names, a closer agreement with the MT than the earlier LXX did.

Now on this point I have found a perusal of Dahse's chapter immensely reassuring. He dis-

* It may be mentioned in passing that Dahse tries to show that the Hebrew used by Origen differed in one or two instances from our Massoretic text. If the difference could be proved in several cases, it would certainly be an important fact; but it would not prove that Origen's Hebrew text was independent of the Massoretic. It might only mean that he relied on a carelessly written† MS. of that text. That he followed a *recension* different from the Massoretic, or even a text materially at variance with it, is a position which I do not think any authority on the LXX would maintain.

† I leave the expression "carelessly written" because Dahse in his Reply (p. 492) makes it the object of sarcastic remark. It would have been better to say "divergent." See p. 77 f. below.

cusses in all about forty-four readings out of some 320 divine names in Genesis. In the great majority of cases the Hexaplaric influence appears in the conflate reading κύριος ὁ θεός which is found in different MS. groups. Dahse clearly shows that in several instances this reading arises through copying the Hexapla with omission of the critical signs, in the way illustrated above; and of course in all such cases the presumption is that the name which *differs* from the MT represents the original LXX. If we may assume that the examination is fairly exhaustive of the traces of Origen's work in the divine names (and I see no reason to suppose otherwise) the influence of the Hexapla has been much more restricted than might have been expected. But we can go much further than this. After all, it is of little interest to us in the present controversy to know that the effect of Origen's work can be traced in this or that MS. or group of MSS., or in this or that secondary version. The real practical question is how far it has affected what may be called the standard text of the LXX, as represented say by the Cambridge Septuagint, which always follows the best available uncial. Not, be it observed, because that uncial is necessarily the best witness to the original text of the LXX; but because the edition affords a convenient standard of primary reference in all comparisons of the various types of text. Or, coming nearer home, the question is whether the statistics given in the synopsis in the last chapter are vitiated by uncertainty as to the extent to which the readings there adopted have

been assimilated to the MT through Hexaplaric influence. And here Dahse's results are still more reassuring. He examines only twenty-four readings * in chaps. xii.–l., and in sixteen cases he decides in accordance with the standard LXX. Only in seven or at most eight cases does he prefer another reading: viz., in xiii. 4 (?), xv. 4, xvi. 11, xviii. 1, xviii. 14, xxi. 4, xxiv. 40 (?), xxvii. 20. I am bound to say deliberately that in my opinion the reasons given for the preference are in every instance (except xvi. 11) of negligible value; but even if we accept them all the difference is inappreciable. Moreover the eight passages were all noted in the third line (or in the footnotes) of the tables in the article referred to. It would appear, therefore, that no misgiving need be entertained as to the possible effect of the Hexapla in invalidating the argument already advanced against the pericope-hypothesis. With that satisfactory finding our present interest in the Hexapla of Origen ceases.

### 2. *The Recension* egj.

We come now to a group of MSS., bearing evidence of descent from a common archetype, which Dahse identifies with the Hesychian recension. The leading representatives of the group are three cursives, dating from the tenth to the fourteenth century, whose symbols in the apparatus of the Cambridge LXX are the letters **e**, **g** and **j**. The

* xii. 17; xiii. 4, 10, 13, 14; xv. 4, 7; xviii. 1, 14; xix. 16*b*,*c*; xx. 11; xxi. 2, 4, 6; xxiv. 40; xxv. 21*b*; xxvii. 20 xxix. 31, 32, 33; xxx. 30; xxxviii. 7*b*, 10*a*.

main stock of the recensions, we are told, is **ej** ; **g** frequently parting company with these two. But there is also a considerable number of MSS., more or less closely affiliated with the group, which can be used by an expert critic to ascertain the distinctive readings of the lost archetype. With regard to these, and the general character of the recension, we get no information in the volume before us ; but are referred to an earlier paper of Dahse's in the *Zeitschrift für die alttestamentliche Wissenschaft* for 1908 (p. 13 ff.). It will be seen how impossible it is for any one who has not minutely worked over the whole ground to control or verify the readings assigned by Dahse to this recension, and I frankly confess my inability to do so. For my present purpose it will be sufficient if I accept provisionally his determination of the text of the recension. Needless to say, however, I am not prepared to extend even a provisional confidence to all the conclusions which he deduces from the data I shall assume him to have established.

1. Let us inquire, then, in the first place, what are the characteristic tendencies of the recension in regard to the use of the divine names. We read (p. 107) that in our recension the tendency is observable "to use only one and the same name for God in one section." Two readings (iv. 5 and vi. 3) are expressly excluded on account of their uncertainty ; and then we get lists of cases where (1) *κύριος ὁ θεός*, (2) *κύριος* and (3) *ὁ θεός* occur in accordance with this principle of assimilation. (1) *κύριος ὁ θεός* is quoted as characteristic of the recension in ten passages : iii. 1*b*, iii. 11, iv. 13,

vi. 13, vi. 22, ix. 17, x. 9*a*, x. 9*b*, xiii. 4, xvi. 7. But in iv. 13, vi. 13, vi. 22, x. 9*a*, *b*, xvi. 7 the double name is the reading of the general text of the LXX (in iv. 13, vi. 13, x. 9*b*, xiii. 4, *perhaps* vi. 22, it seems clearly Hexaplaric), so that from these instances nothing can be inferred as to the *special* tendencies of **egj**. Hence there remain only three clear cases (iii. 1*b*, iii. 11, ix. 17) to support Dahse's sweeping generalization. Then what is meant by a "section" (*Abschnitt*)? It cannot be a Seder, for in Seder 2 (ii. 4–iii. 21) ὁ θεός occurs no fewer than six times in our recension (ii. 4*b*, ii. 9, ii. 19, ii. 21, iii. 3, iii. 5), while Dahse himself only cites two cases of κύριος ὁ θεός (iii. 1*b*, 11) as *characteristic* of it. In Seder 3 (iii. 22–iv. 26) against one case cited (and that *not* distinctive) of κύριος ὁ θεός (iv. 13) we have ὁ θεός five times (iv. 1, iv. 4, iv. 10, iv. 16, iv. 25) and κύριος once (iv. 3). We need proceed no further on *that* trail. Perhaps Dahse's real meaning is better expressed by the vaguer phrase "in the same context" (p. 107). He says (p. 106) that "between vi. 12 and vii. 1 ὁ θεός never occurs alone in **ej**, but only κύριος ὁ θεός." Considering that between vi. 12 and vii. 1 the divine name occurs only twice (vi. 13, 22), and that in vi. 22 κύριος ὁ θεός is the common reading of the LXX (as also in vi. 12, vii. 1), it does not seem a very impressive exhibition of consistency that *once* (vi. 13) **ej**, *following the Hexapla*, reads the double name. Again, "a solitary κύριος appears in the group only once (iv. 3) in the first ten chapters of Genesis." And how often does the reader imagine that (ὁ) κύριος occurs alone in these ten chapters in the standard text of the

LXX? Just three times (iv. 3, iv. 13, x. 9*b*),* and in the last two of these the double name is probably Hexaplaric, and is at any rate the most prevalent LXX reading. So much for *κύριος ὁ θεός*. We are invited further to find illustrations of the tendency (2) in the *κύριος* of xii. 17, xix. 29*a*, *b*, and (3) in the *ὁ θεός* of viii. 20, xv. 4, xx. 18. (2) It is true that in xii. 17 the group changes *ὁ θεός* into *κύριος* between two readings of *κύριος* (xii. 8 and xiii. 4) and similarly in xix. 29*b*; but in xix. 29*a* the *κύριος* is common to all MSS. of the LXX except 9 (E omits). In this last case the change does bring about a uniform use of *κύριος* throughout a whole Seder; but apart from xix. 29*b* the uniformity exists already in the LXX: in xii. 17 no such consistency results, *ὁ θεός* remaining in xiii. 10*a*, *b*, 13, 14. (3) On viii. 20, we read (p. 104), "the MT after three times אלהים (viii. 1*a*, 1*b*, 15) has in *v.* 20 יהוה, which our group changes to *ὁ θεός*." True, but "our group" in viii. 15 has *not* *ὁ θεός* but (in common with the entire LXX except one MS.) *κύριος ὁ θεός*, which breaks the sequence. In xv. 4 Dahse holds, on the evidence of six cursives and the Old Latin, that no name stood after *φωνή* in the original LXX, that *κύριου* was inserted by the Hexapla (in spite of the fact that *τοῦ θεοῦ* is read by two daughter versions of the LXX, the Armenian and Sahidic,† of which the former is strongly Hexaplaric), while **egj** with others insert

* Dahse (p. 38) omits iv. 13, but adds viii. 20. The truth is that both in iv. 13 and viii. 20 the reading is very weakly attested. See the Note on *ὁ κύριος* readings at the close of this section.

† Not the Ethiopic, as Dahse says.

τοῦ θεοῦ. If we accept his view the name corresponds with the two which follow (ὁ θεός) and differs from the three which precede (κύριος): we see that whichever name was inserted it could not fail to agree with either the one or the other. In xx. 18 κύριος is changed to ὁ θεός in harmony with all the other names of Sed. 17. To the same effect we read (p. 104 f.) that in ix. 17 "members of our group have κύριος ὁ θεός following the double name in ix. 12, just as in iii. 11 between iii. 10 and iii. 13, and vi. 13, 22 between vi. 12 and vii. 1." This is true (but on vi. 13, 22 see above); but the next statement is misleading; "in xi. 5 begins in it (the recension) the *continuous* appearance of the solitary κύριος." In the very next verse (xi. 6) ej have κύριος ὁ θεός; and although with that exception the reading κύριος is continuous to the end of Sed. 8 and throughout Sed. 9, the recension simply follows the main current of the LXX text.*

Dahse further calls attention to the fact that the group has important readings in v. 29, xx. 4, xxvi. 29, xxviii. 20, xviii. 27, xxxii. 9. In v. 29 its peculiarity is the addition of ἡμῶν to the κύριος ὁ θεός of the ordinary LXX, and I do not know in what its importance consists. In xx. 4 for the אדני of the MT the recension has κύριε ὁ θεός, which Dahse very arbitrarily holds to imply a double name יהוה אדני or אדני יהוה † in the original. xxvi. 29

* xi. 8, 9*a*, 9*b*; xii. 1, 4, 7*a*, 7*b*, 8*a*, 8*b*.

† In xv. 2, 8 the rendering of אדני יהוה is δέσποτα κύριε, or (with insignificant exceptions) δέσποτα alone. In xv. 2 the κύριε is marked as a Hexaplaric insertion. And why it should be necessary to postulate the double name as Hebrew basis of κς ὁ θς in this solitary instance (xx. 4) passes comprehension.

should have been mentioned as a glaring exception to the general tendency of the recension, inasmuch as it breaks a long sequence of κύριος by a solitary ὁ θεός: its supposed importance lies in the fact that in the speech of a heathen king, Abimelech, **egj** substitute *Elohim* for *Yahwe* of the MT and LXX. In xxviii. 20 the group preserves the κύριος (see above, pp. 42, 53 f.) which Dahse regards as the reading of the original LXX (MT *Elohim:* LXX κύριος ὁ θεός). In xviii. 27 it omits (in common with the great majority of LXX MSS.) after τὸν κύριον a μου which is read by the Bohairic and Sahidic versions and eight cursives. Dahse infers that it represents not אדני (MT) but יהוה in the Hebrew. If so, must we not conclude that the main text of the LXX does the same? Finally in xxxii. 9 the recension adds ὁ θεός to the κύριε (MT יהוה) of the ordinary LXX, to which however the MSS. present variants κύριε ὁ θεός μου, ὁ θεός, and others. It should be stated that in xx. 4, xxviii. 20, xviii. 27 (also iii. 1*b*, vi. 13 (?), xviii. 31, xix. 29) there are variants in Hebrew MSS. which are thought to enhance the significance of our recension. To this subject we shall return presently.

It is difficult to form a clear judgment on these conflicting phenomena as evidencing a special tendency of the recension **egj**. In order to do so we should have first of all to isolate the group from the common text of the LXX, and then to understand how the influence of the Hexapla, which Dahse expressly emphasizes, was brought to bear on the recension; and in neither direction is Dahse's work helpful. I will state

only two impressions. (1) It seems fair to say that this recension goes a little, but only a little, beyond the ordinary LXX in assimilating a name to those in the immediate context. I can recognize this leaning in at most seven passages (x. 9*b*, xii. 17, xv. 4, xviii. 27, xix. 29*b*, xx. 18, xxviii. 20*b*); but the opposite also occurs (xi. 6, xviii. 20, xxvi. 29). Here the question arises whether these instances are sufficient to prove deliberate purpose on the part of the author of the recension. It seems to me that they are adequately explained as unconscious adaptations to the nearest divine names. One cannot help wondering whether Dahse has ever considered this possibility. (2) The peculiarities of the recension in the use of the divine names are entirely explicable on the supposition that it originated within the sphere of the Greek text. In other words, apart from agreements with Hebrew MSS. (which we have yet to consider), there is nothing whatever to suggest that the changes are determined by reference to a Hebrew original different from that which lay behind the LXX. I do not admit that the addition of μου is a criterion for אדני as distinct from יהוה in the Hebrew (xviii. 27, xviii. 31): it can be naturally accounted for as an inner-Greek insertion suggested by the invariably vocative use of the word,* and in any case

* The ten cases (אדני in xviii. 3, 27, 30, 31, 32; xix. 2 (pl.); xix. 18; xx. 4: אדני יהוה in xv. 2, 8), where אדני occurs in MT, are all literally or virtually vocatives; and the μου is never found in the prevalent text of the LXX. But it occurs four times in Boh. and Sah. (xviii. 3, 27,

xviii. 27 would be the only instance of the kind where **egj** agrees with Hebrew MSS. against the MT.

2. This brings us to the most important question of the supposed Hebrew basis of the recension **egj**. As we have seen, the proof of this is sought in agreements of the recension with a group of Hebrew MSS. With the general subject of variants in Hebrew MSS. I shall deal more fully in the next chapter: here it is only necessary to consider the coincidences between **egj** and the particular MSS. which are said to support it.

We may start from xx. 4, where nine MSS. of Kennicott (9, 81, 132, 150, 152, 199, 227, 239, 601) and five of De Rossi (419, 455, 507, 766, primo 248) read יהוה instead of MT אדני. Now it is certainly a most unusual thing to find a nest of Hebrew variants like this to any Massoretic reading of the divine name in Genesis. But it must be observed that it is just in the case of יהוה ‖ אדני that variations in Hebrew MSS. most frequently occur. The reason is not far to seek. אדני and יהוה were *pronounced* alike by later Jews (*Adonay*), and the scribe, whether writing from dictation or (according to a copyists' rule) pronouncing each word before setting it down, very readily con-

31; xix. 2); twice in Eth. (xviii. 3; xix. 18); and four times in a few cursives other than **egj** (xviii. 27, 31; xix. 2, 18). Dahse may of course maintain either (*a*) that the original LXX read יהוה in all these places, or (*b*) that the μου is original and has dropped out of the current text; but neither view is probable.

fused the two names in writing. But curiously enough in xx. 4 the MSS. cited do *not* support **egj**, for **ej** read *κύριε ὁ θεός*,* which, according to Dahse, implies an original אדני יהוה or יהוה אדני, while **g** (with all other MSS.) reads *κύριε*. That is not a very promising beginning for the theory of a Hebrew basis. But we must inquire further whether these nine MSS. of Kennicott form a true "group," as Dahse says they most assuredly do. The presence of nine men in a tavern on one occasion is scarcely presumptive evidence of a conspiracy, though if they are frequently found in company the suspicions of the law may be aroused. Now (1), so far as Dahse's tables inform us, no two of these nine MSS. are ever found together again leagued against the MT except in xv. 2, where 150, 152 read יהוה אלהים, for MT אדני יהוה,† and in xviii. 31, where 227, 239 read יהוה for אדני; and in neither case does **egj** support them. (2) Only two of them ever support **egj** even singly against MT anywhere: viz., 132 in iii. 1*b*, xviii. 27, and 199 in xix. 29*a*.‡ (3) Over against

* See footnote, p. 72.

† Observe again that both these phrases were pronounced alike: *Adonay Elohim*.

‡ I exclude vi. 13 because I do not believe it is a genuine case. K152 there reads יה אלהים, and Dahse, following Wiener, takes the first word to be shortened form of יהוה: this would agree with the *κύριος ὁ θεός* of **ej**. I have not seen the MS., but I have little doubt that the יה is a copyist's error: the scribe had begun to write יהוה, but after forming two letters he noticed that the right word was אלהים, which accordingly he wrote without removing the traces of his mistake. A

these three coincidences of **egj** with this alleged group of Hebrew MSS. against MT, there are about thirty cases* where **e(g)j** differ from MT without any support from the group, and except in xxviii. 20*b* without any Hebrew support at all. If that be sufficient to prove that a recension "goes back" to a Hebrew original, textual criticism ceases to be an exact science.

There are some other matters that require clearing up. What is meant when it is said (p. 107) that the recension "goes back" to a Hebrew original? Dahse cannot possibly mean that it is a fresh translation from the original, though his words might convey that impression to an uninstructed reader.† All that can be intended is surely a *correction* of the Greek text by comparison with the Hebrew recension in question, and we have seen how slight is the evidence that any such comparison was ever made. But supposing for the sake of argument that it did take place, a single Hebrew MS. would suffice for the purpose, and it is unlikely that the reviser will have used more. We should, therefore, in the assumed case have a parallel to what we have conceded as possible in the case of Origen's Hexapla: viz., the use of a MS. representing the

similar confusion in K109 (on ch. xviii. 27) will be considered in the next chapter.

* iii. 11, iv. 13, v. 29, vi. 13, vi. 22, viii. 20, ix. 17, x. 9*a*, x. 9*b*, xi. 6, xiii. 4, xv. 4, xvi. 7, xviii. 13, xviii. 20, xviii. 22, xviii. 26, xix. 16*a*, xix. 29*b*, xx. 8 [xx. 18], xxi. 1*a*, xxi. 2, xxi. 4, xxi. 6, xxvi. 24*a*, xxvi. 29, xxviii. 20*b*, xxxii. 10. In xx. 18 **ej** agree with Sam.

† See p. 243.

Massoretic recension, but containing variations (such as virtually all MSS. contain) which might be either superior or inferior to our present Massoretic text. There is no occasion to call in the theory of an independent Hebrew recension.

Another point to be noticed is that on p. 107 Dahse puts this recension egj between the original of the LXX and the MT, implying that the hypothetical Hebrew basis of egj is older than the latter. But if it be older than the MT it must represent a distribution of the divine names older than the Samaritan Pentateuch; and the first literary trace of it is in Greek codices of the tenth century. What likelihood is there that an unofficial recension should have retained its characteristic features in a recognizable degree of purity through twelve centuries of transmission in Hebrew and Greek MSS., especially in so variable an element of the text as Dahse supposes the divine names to be?

### 3. *The Recension* fir.

These three MSS., assigned respectively to the 15th, the 11th, and the 13th century, form the "groundstock" of a recension which, as we have seen, Dahse identifies with that of Lucian. We have also seen that this identification is considered by other scholars to rest on very precarious grounds. In the chapter now before us Dahse seeks to prove that the group represents an "Elohistic edition of Genesis" (p. 114); and we have to try and see how far that description is

appropriate. The statistics given below* are based on Dahse's examination, and are at least approximately correct. It will be seen that in about half the passages examined the recension agrees with the common reading of the LXX. Although we must not assume in argument that the prevalent form of the LXX is older than the recension, it is at the same time impossible to investigate the peculiarities of a particular recension otherwise than by comparison with the general characteristics of the LXX; and until these have been finally ascertained we must use some standard of reference, such as the Cambridge edition. Bearing this in mind, we find that though the recension does show a very decided preference of ὁ θεός to κύριος, it shows a still greater partiality for κύριος ὁ θεός over κύριος and

* In the cases in which Dahse comes to a definite conclusion the recension reads:—

| | | In agreement with LXX | For κύριος | For ὁ θεός | For κύριος ὁ θς | + | — |
|---|---|---|---|---|---|---|---|
| 1. ὁ θεός | 27 times, viz. | 14 | 9 | — | 3 | 1 | — |
| 2. κύριος | 44 ,, ,, | 35 | — | 1 | — | 5 | 3 |
| 3. κύριος ὁ θεός | 45 ,, ,, | 7 | 28 | 9 | — | 1 | — |
| | 116 | 56 | 37 | 10 | 3 | 7 | 3 |

The MSS. of the recension are frequently at variance, and even Dahse has often to confess himself uncertain what name really belongs to it. That he is invariably right when he expresses no hesitation is probably more than he himself would claim.

even over ὁ θεός. Thus while κύριος ὁ θεός is only three times changed to ὁ θεός and never to κύριος, it is twenty-eight times substituted for κύριος and nine times for ὁ θεός. Further, though κύριος is nine times changed to ὁ θεός and twenty-eight times to κύριος ὁ θεός, in thirty-five cases it is allowed to stand. These facts are a serious set back to Dahse's theory of an Elohistic recension. It is of no avail to point out, as Dahse does, that in five passages the retention of κύριος is explained by its occurring at the end (xviii. 33, xxvii. 27) or beginning (xxxviii. 7, xlix. 18) of a Seder, or (xxi. 6) at the end of a pericope in an ancient Christian lectionary (!); or again, that in some half-dozen instances it follows "angel" or "name": there are thirty-five to be accounted for. If finally it be alleged that the predominance of κύριος ὁ θεός is itself evidence of an elohistic tendency (ὁ θεός being *added* to an original κύριος), we have to ask why κύριος, though changed to ὁ θεός in nine cases, is nevertheless retained alone in no fewer than thirty-five, and further how it comes about that κύριος ὁ θεός appears nine times in place of ὁ θεός. It seems clear that no principle is consistently followed by the author of the recension in his use of the divine names, or, if there be, that Dahse has not detected it. So far as the interchanges of ὁ θεός and κύριος are concerned, the facts could be adequately explained by the natural predilection of Greek writers for ὁ θεός being carried somewhat further in this case than in the main text of the LXX. But it must be admitted that the preference for κύριος ὁ θεός cannot be satisfactorily accounted for in this way. It might no

doubt have come in through conflation at a later stage of the text than the recension fir; and if so, it seems impossible with our present knowledge to determine which component was found in that recension.*

The grounds on which Dahse postulates a Hebrew basis for the recension fir in its use of the divine names are as unconvincing as could well be imagined. In the first place, he points to a single agreement with K650 in xlii. 5. It is true that Kennicott cites 650H as reading לשבר בר for the Massoretic לשבר; and similarly fir read (with the Sahidic version) *ἀγοράζειν σῖτον* for the bare *ἀγοράζειν* of the LXX. But לשבר בר occurs immediately before in *v.* 3, and there also the *σῖτον* appears in all LXX codices. It would not have been very wonderful if one Greek and one Hebrew copyist had both supplied the accusative from the preceding context without collusion or interdependence. And even if dependence of the one on the other were probable, would that be sufficient evidence for the existence of a whole Hebrew

* Dahse (p. 114) promises a fuller discussion of the *κύριος ὁ θεός* readings in a further volume of his textual studies. Meanwhile he appears to hold to the opinion, based on a doubtful interpretation of a statement of Jacob of Edessa, that it was the practice of Lucian (the supposed author of our recension) to combine the marginal reading of the divine names with that of the text of the MSS. which he followed. In that case there would have been over sixty readings to which he found no margin; and we are left with thirteen absolute substitutions of one name for another which are only explicable by the tendency of Greek scribes spoken of above. There is not the slightest reason to suppose that either text or margin represented a Hebrew original.

recension in which the divine names were treated on different principles from the MT? But it is really wasting time to speculate about such probabilities; for the fact is that K650 is not a MS. at all, but a *printed edition*, and that not of the Pentateuch but of the Talmud! (see Kennicott, *Dissertatio generalis*, p. 108). The reading has no value whatever; it is simply one of those cases of inexact citation from memory which abound in the Talmud, and for which there is no reason to assume any MS. authority. But in the second place, Dahse asserts that "the Elohistic tendency has had regard to the Sedarim-division, while the author of the recension ignores this." It is difficult to apprehend so very refined a distinction. It would appear to be Dahse's view that in fir we have to do with a double recension: first a recension of the Hebrew text, in which some attention was paid to the Sedarim-division, and then a Greek recension in which the Sedarim were ignored. How does he manage to accomplish such an extraordinarily subtle critical operation? (*a*) As an indication of regard to the Seder-division he has pointed out the occurrence of a κύριος *twice* at the beginning and *twice* at the end of a Seder. We have seen already how little importance can be attached to that observation. But even supposing it to be significant, does it prove the existence of a *Hebrew* original? Were we not given to understand at an earlier stage of the argument that in Dahse's opinion a regard to the Sedarim was characteristic of the original LXX as a whole? How then can he tell that the text which the "author of the recension" had before him was anything but a

Greek MS. of the LXX? (*b*) How does he know that the "author of the (Greek) recension" disregarded the Seder-division? He says that when the reviser supplies out of his own head a name not found in his original (vii. 23, xviii. 19*c*, xxii. 9, xxvi. 25*a*) he is careless what name he chooses, and thus betrays indifference to the prevalent usage of the section before him. Again, I am unable to perceive in that any ground for believing that his original was in Hebrew. But whether it was Hebrew or Greek, so long as it was a recension independent both of the MT and the original LXX, who is to tell us that in the passages cited the names were not found, but were supplied by the second reviser? We know what names were in the MT and in the current LXX; and in all the four passages here referred to* these two texts agree in having no divine name at all. But as to what names were or were not in a speculative Hebrew recension of which not a trace has survived, Dahse can have no knowledge whatsoever. There is no conceivable reason why the alleged recensional additions should not have been made to the Greek text of the LXX; and the whole argument merely shows on how frail a foundation Dahse builds his imposing but unsub-

* We might add iii. 24, xx. 8, xxviii. 13*b*, 20*a*. Dahse also instances vi. 14 (p. 108); but that must be a mistake: there is no name in that verse. It detracts considerably from the form of Dahse's generalization when we observe that **fir** goes its own way only in vii. 23, xviii. 19*a*. In iii. 24 it agrees with (practically) the entire LXX, in xx. 8 with **e(g)j**, in xxii. 9 with **b** *al.*, in xxvi. 25 with **Edpt** *al.*, in xxviii. 13 with the Hex., and in xxviii. 20 with **dp**.

stantial theory of Hebrew recensions differing from the Massoretic text.* "It is true, in general," writes Professor Torrey, of Yale, "of the modern use of the Greek Bible for text-critical purposes, that recourse is had far too often to the hypothesis of divergent Hebrew texts, while there is far too little appreciation of the extent to which the Greek texts themselves have been corrupted in transmission." † Certainly in Dahse's critical practice we see that tendency carried to most unwarranted extremes.‡

* For a further discussion of these two recensions, see p. 246 ff.; and NOTE IV, p. 271.

† *Ezra Studies*, p. 109.

‡ *Note on the ὁ κύριος readings.*—The name יהוה is ordinarily rendered in LXX by κύριος without the article. In nearly a score of instances, however, we find ὁ κύριος; and the question suggests itself whether the distinction has any significance. In regard to three cases (iv. 3, 13; viii. 20) Dahse (p. 38 f.) offers the explanation that ὁ κύριος is used to signify that "in matters of cultus one addressed oneself not to any Elohim indifferently, but to Yahwe." That is an echo of Eerdmans' theory of a polytheistic phase of the Genesis legends, of which Dahse makes a somewhat unfortunate application. He appears to overlook the fact that the presence or absence of the article is a peculiarly Greek feature which has no expression in Hebrew, and therefore must be traced to the translators or later copyists. But the translators of the LXX were far removed from the stage of thought at which it might have been necessary to guard against a polytheistic sense of Elohim. Dahse does not inquire whether the principle holds good in all or most of the other cases; nevertheless his general idea has some justification in actual usage. The facts are these: (*a*) ὁ κς is used for אדני twice (xviii. 27, 31): now in all other instances of אדני it is represented by a vocative; hence we may say that ὁ κς is the regular equivalent of אדני wherever the art.

is admissible. (*b*) For יהוה, ὁ κς stands in iv. 3, 13, viii. 20, xii. 8*a*, xiii. 4, 18, xvi. 2, xviii. 17, 33, xxiv. 16, 48*a*, 52, xxviii. 13*a*, xxxix. 23*a*. Of these iv. 13, xviii. 33 (and perhaps viii. 20) may be set aside as insufficiently attested, but as illustrating a tendency they are here reckoned. Of the fourteen cases no fewer than nine (iv. 3, iv. 13, viii. 20, xii. 8*a*, xiii. 4, 18, xxiv. 26, 48, 52) refer to acts of worship; and we may add xxii. 9, where a few authorities supply τῷ κῳ after "altar." On the other hand there are many references to worship (e.g., xii. 8*b* !), where ὁ κς is not used. The result can hardly be set down to chance; although at the same time the element of chance appears in the five cases above, which have nothing to do with worship (xvi. 2, xviii. 17, 33, xxviii. 13, xxxix. 23), as well as in several variants which are not included.—Dahse does not point out that a slight tendency to favour ὁ κς is observable in egj. The fact goes to show that that recension is not based on a Hebrew original.—See NOTE V, p. 273.

## IV

# THE HEBREW TEXT

IT has already become apparent that the discussion in which we are engaged involves a conflict between two diametrically opposite points of view. Dahse, bringing to the investigation the prepossessions natural to a student of the LXX, is profoundly impressed with the instability of the textual tradition as regards the transmission of the divine names. In the LXX, fluctuation is indeed a conspicuous feature of the *apparatus criticus*; and it is perhaps true to say that in the Greek Pentateuch no element of the text is so liable to variation as the names for God. But Dahse seems to realize, more clearly than other writers of his school, that the diversity of the Greek text does not go far to prove the unreliability of the names as a whole, unless he can succeed somehow in drawing the Hebrew tradition into the vortex of confusion which exists in MSS. of the LXX. Hence he has laboured to show in the first place that the peculiarities of the Greek version are due not to accident or caprice, but to systematic alterations governed by a regard to the divisions of the Synagogue lectionary; and secondly, that its variations are based in part on different Hebrew recensions, which are entitled

to quite as much consideration as the standard Massoretic recension. These positions of his have been examined at some length in the two preceding chapters, where I venture to think I have shown that he is wrong all the time. If the arguments there adduced are conclusive, we might almost at this point wash our hands of the LXX altogether. It might safely be left, with its multiformity of text, in Dahse's hands to make what he can of it; and whether he discover a method in its madness or not is henceforth of very little consequence to us. It is purely a matter of the internal condition of the Greek text, which in no degree affects the question of the stability and trustworthiness of the Hebrew tradition.

The view represented in this volume, on the contrary, is that the divine names are a remarkably stable element of the text. It is fair to admit that this impression rests in the first instance on the solidarity of the Hebrew text, although it is decidedly confirmed when we take into account the evidence of all versions other than the LXX. No one contends that the Hebrew text enjoys perfect immunity from error, or that it preserves with unfailing accuracy the names as they occurred in the original autographs of the sources of Genesis. The possibility of error in the Hebrew text must be recognized; all that is necessary for the justification of the critical use of that text as a guide to the separation of documents is evidence that the range of error is restricted within such narrow limits that it cannot seriously affect conclusions based on the assumption that the MT is correct. We shall see at

a later stage that the versions, always excepting the LXX, differ so little from the MT as to confirm the impression that the divine names have been transmitted with peculiar fidelity. We may not be sure in regard to each particular name that it stands as it did in the primary document; but we may nevertheless find reason to believe that this must be the case in a sufficient number of instances to furnish a sound basis of induction, and to form the starting-point for a documentary theory of the Pentateuch. It will be the chief object of the remainder of my argument to uphold the thesis that in the MT we have a recension of the divine names which possesses this character of stability in a remarkable degree, one which has undergone no material variation for more than two thousand years, and which therefore may fairly claim to represent, at least approximately, the names that stood in the original Genesis, or in the documents of which it was composed. The direct vindication of this position must, from the nature of the case, follow two lines of argument. We cannot hope to reach an absolute demonstration that the Hebrew text *never* varied in its transmission of the names of God, or that in the unknown earlier stages of its history it possessed the rigid uniformity which is observed in its more recent development. But (1) we can show that the evidence adduced by Dahse and others in proof of its variability is of no value, because it ignores the fundamental canons of Massoretic criticism; and (2) we can point to facts which give a reasonable assurance that the present distribution of the divine names

goes back in the main to a time not very much later than the final redaction and canonization of the Pentateuch. In the first line of proof we are concerned with the evidence of Hebrew MSS.; in the second with the problem of the Samaritan Pentateuch.

### 1. *Hebrew Manuscripts.*

The received text of the Hebrew Bible lies before us in a considerable number of printed editions, and in some two thousand extant MSS. of the Old Testament in whole or in parts. All these exhibit substantially one and the same text. As regards the divine names of the Pentateuch, I do not know whether in the printed editions there are any variations at all. In the extant codices occasional variations do certainly occur; and it is the importance of these that we have now to consider. It is the unscholarly practice of writers like Wiener and Schlögl, unfortunately followed by Dahse, that while reproaching the higher critics for their neglect of MS. evidence, they cite MS. variants indiscriminately, without apparently having taken the least pains to inform themselves (and certainly no pains at all to inform their readers) of the date and value of the codices in question, and without even considering the proportion of differences to agreements which are found amongst them as compared with the standard text. Now, in point of fact, there is some excuse for disregarding Hebrew MSS. entirely; but there is none for arguing as if one MS. were as good as another, or as if a single variant in one or two MSS. were enough of itself

to throw doubt on the soundness of the received text. To make this clear it is necessary to explain at some length certain facts about the history of the Hebrew text which are constantly overlooked by the class of writers to whom Dahse adheres.

1. How, it may be asked, can it ever be right, or even excusable, to ignore the evidence of accessible manuscripts? A general answer to that question might be that the MSS. vary so slightly, and in such unimportant minutiæ, that it is hardly worth while, except in special cases, to consult them or to investigate their differences. But that is not the main reason for assigning a relatively small importance to the variants found in codices of the Hebrew text.

(1) The leading fact is that for the last eighteen centuries at least there has existed a recognized *standard text*, which has been the norm by which the correctness of all MSS. has been judged. Of course the standard text is represented only by MSS. and (since the fifteenth century) in printed editions; but the consensus of MSS. does not constitute its sole or chief authority. Its transmission has been carefully guarded by a succession of official custodians, at first by the Sopherim or scribes, and later by those known as the Massoretes; and these authorities have sought to regulate it and maintain its purity, not merely by extreme care in the copying of MSS., but still more by the invention of the elaborate system of rules and observations which is called the *Massora* (= "tradition"). Many of these observations go back to a remote antiquity (some probably to pre-Christian times); most of them perhaps

date from the flourishing period of the Massoretes, from about the sixth to the tenth century; but the development and expansion of the system was not arrested till the introduction of printing towards the end of the fifteenth century. That the scheme was not entirely successful appears in the fact that in spite of it slight differences do occur in MSS.; that it was very nearly successful is shown by their surprising unanimity. The result is that in countless cases we know quite certainly, apart from MSS. altogether, what was the text which was deemed correct by the authoritative exponents of the Jewish textual tradition: and since extant MSS. are all of later date than the great age of the Massora, we can be sure that where any MS. violates a Massoretic injunction it goes against the best Jewish professional opinion, and is therefore presumably a clerical mistake. Now this standard text, guaranteed by the Massora, is represented with substantial fidelity, and in the case of the divine names with perfect fidelity, in all printed Hebrew Bibles; so that whatever edition the student happens to use he may feel a practical certainty that he has before him the divine names in the most authoritative form of the Hebrew text which we can now by any possibility attain.*

* In illustration of the bearing of the Massora on the use of the divine names I may here instance two rules which Dahse quotes on p. 11, and which in his opinion should have prevented me from writing as I did in a brief note on the occurrences of אדני יהוה (*ICC.* p. 278). The first is, "In the Pentateuch and the Hagiographa the reading is always יהוה אלהים, only in 8 cases אדני יהוה" (Genesis xv. 2, 8; Deuteronomy iii. 24, ix. 26; Psalms lxix. 7, lxxi. 5, 26, lxxiii. 28),

(2) This standard text has existed in several forms which by courtesy are called "recensions," although their almost imperceptible divergences scarcely entitle them to that designation. First of all, nearest to our own time, we have the two divergent "recensions" of Ben Asher and Ben Naphtali, dating from the tenth century, the former of which is followed almost exclusively by European MSS. and in the printed editions. These, however, differ only in the vowels and accents, and agree in the consonantal text. Somewhat more important is the older rivalry between the Eastern (Babylonian) and Western (Palestinian

The other is, "In the Prophets אדני יהוה is always to be written except in five passages, where the reading is יהוה אלהים." It is of course true that such rules tended to suppress variants in MSS.—that is what the Massora is for—and if amongst these variants there were one older than the standard recension it would be suppressed along with the rest. On the other hand it must be remembered that these regulations were not constructed by the Massoretes out of their own heads. They are based on the MSS. which seemed to the Massoretes most authoritative, as representing the standard text which they wished to propagate; and their object is to guard against the mistakes into which copyists were apt to fall because of the identical pronunciation of these two phrases (see below, p. 99). The selection of MSS. may not always have been judicious, or the standard text itself may be at fault; and therefore it is perfectly in order to argue (as Dahse here does—although I do not admit that he *proves*) that a different text from the Massoretic is to be preferred. But at present we are dealing simply with the evidence of Hebrew MSS.; and when it is a question between the deliberate judgment of the Massoretes on one side, and the variations of one or two MSS. on the other, there can be no doubt that the former is an infinitely better authority for the official Hebrew text than the latter.

and European) "recensions." Of the former, many MSS. have come to light during the last seventy years; but besides these we have ancient Massoretic lists of the readings in which the two differ. It is found that in the Pentateuch there are practically no consonantal variants: in Genesis, for example, the only discrepancy is in the spelling of Tubal-Kain as one word or as two (see Baer's *Genesis*, p. 81): there are of course none in the divine names. Thus from the unknown time when the Eastern and Western texts divided, there has been no authorized variation in the transmission of the names for God. In view of this astonishing uniformity, what weight can we attach to the aberrations of a few fourteenth or fifteenth century MSS. belonging to the Western "recension"? Is the presumption not overwhelmingly strong that they are simply scribal errors, which have eluded the precautions taken by copyists, and escaped the vigilant eyes of the Massoretes?

(3) But here a still more surprising and significant fact comes into consideration. The standard text contains stereotyped errors and defects which were recognized as such by those responsible for its maintenance; and also eccentricities which, though not exactly errors, are purely accidental, and have no value in themselves apart from some traditional prejudice.* There are words omitted which are necessary to the sense, and which were accordingly supplied in the reading; and others inserted where they make nonsense, and omitted in reading; words and

* See NOTE VI, p. 274.

letters marked by peculiar dots (*puncta extraordinaria*); letters written too large or too small, or suspended over the line; vacant spaces in the text; and so on.* Yet the scribes and Massoretes, though perfectly aware of these errors, nevertheless endeavoured to perpetuate them with the same assiduous fidelity as the essential elements of the text. How can this singular procedure be accounted for? It is plain that the eccentric phenomena just described must have originated as accidental peculiarities of a single imperfect codex, which for some reason was regarded with such veneration that its very faults were canonized. We are thus driven to the conclusion that some one defective MS. has been adopted as an "archetype" by the authors of the standard Hebrew recension, and that a persistent effort has been made to bring the whole MS. apparatus into mechanical conformity with it. Since the standard text can be traced back to the middle of the second century, it follows that the archetypal codex is at least of

* Thus (to take a few examples at random), in Jer. li. 3, the word for "bend" is erroneously written twice in all Hebrew Bibles, and similarly the word for "five" in Ezek. xlviii. 16: while in Jer. xxxi. 38 the word for "are coming" after "days" has been accidentally omitted: all such irregularities were rectified in the public reading, but the text itself was never corrected. In Genesis iv. 8 the official Hebrew text has an empty space in the middle of the verse, which several of the versions fill up with the words "let us go into the field": this clause, which seems necessary to avoid a hiatus in the sense, has apparently been dropped from the Hebrew text. On the meaning of the extraordinary points, suspended letters, etc., see the next note.

older date than that. Probably it was some highly venerated MS. which had survived the storm of the Roman wars and the rebellion of Bar Cochba, and was accepted on account of its antiquity as the best available norm for the sacred text at the time when the scribes were engaged in forming an official recension of the Old Testament scriptures.

From these facts many of the most distinguished of recent scholars have drawn the very plausible conclusion that all existing Hebrew MSS. have been produced by a succession of slavishly literal transcriptions from the original codex which chance or necessity had elevated to the position of an archetype for the whole authorized recension.* Now, even if we do not accept the archetypal theory in this extreme

* The following passage from Lagarde states the theory in its most complete and rigorous form: "Holding it probable, as I do, that peoples living in close contiguity, like the Greeks, Syrians and Jews of the first Christian centuries, had the same clerical usages, I am led to explain the graphic peculiarities which appear in Hebrew documents precisely as I should explain them if I encountered them in Greek or Syriac books. That is to say, I consider dotted words as deleted, letters standing over the line as inserted afterwards; from empty spaces I conclude that a hole in the parchment or defective tanning had made the skin unfit to be written on, or else that the copyist had been unable to read his exemplar. . . . If now *puncta extraordinaria* and *literae suspensae* in the Hebrew text prove that the copyists had made a slip, and if the *Pesak* (*lacuna*) is due to some accident that had befallen the scribe or the material on which he was writing, it follows that *all MSS. which show these points, suspended letters, and empty spaces in the same places, must necessarily be slavishly accurate transcriptions of the same original.*"

form, it is of great importance, in view of its *partial* truth, to trace its consequences in the region of textual criticism. It is plain that, in so far as it is true, variations in existing Hebrew MSS. have arisen through mistakes in copying directly or mediately from the archetype. It follows further that in the best event we can never gain more from a comparison of Hebrew MSS. than the readings of a single imperfect codex, to whose authority all earlier types of Hebrew text have been ruthlessly sacrificed. It is conceivable, no doubt, that a minority of MSS. may in some cases have preserved the text of the archetype, while the majority have departed from it. But as regards the divine names that consideration hardly comes into play; for here the variants are so feebly attested that it would be sheer perversity to assert their superiority to the immense preponderance of MS. authority.

For myself, however, I am free to confess that I am not so satisfied of the truth of the extreme form of the archetypal hypothesis as I was at one time. For reasons which need not here be gone into, I have come to think that, while the influence of a single archetype is undeniable, it has been brought to bear on the current text not solely by the way of slavish copying, but partly through the operation of a set of Massoretic rules taken from the archetype and applied in the writing and correcting of MSS. Hence we must allow for the *possibility* that some readings which are older than the official recension have survived as MS. variants; and it is possible that some of these have managed to slip through the ever

narrowing meshes of the Massora and appear in late codices. That must be admitted as a possibility. But on the other hand, there is usually a greater probability that the variations have come in through mistakes in transcription since the establishment of the standard recension. Unfortunately, in the case of the divine names, we rarely have any criterion by which the two kinds of variants can be distinguished. Apart from the occasional support of ancient versions—a point to be considered below—there is always a presumption (considering the general stability of the transmission of the names) that a difference is due to the error of an individual scribe. Thus in this case we are for the most part shut up to one or other of two alternatives: either we must maintain the variant of an insignificant minority of MSS. as the original reading of the standard text, or we must dismiss it as of no importance whatever. Seeing that we very seldom have more than from one to five MSS. agreeing against the majority, there can be little hesitation in deciding on the latter as the only reasonable course.

2. After this lengthy but I hope not irrelevant disquisition on the general problems of the Massoretic text, we must now condescend to particulars. And to give my opponents the benefit of every possible doubt, I have set out in Table VI *all* the Hebrew variants which I have been able to collect. I do not guarantee the completeness of the list; but I think I can vouch for its accuracy so far as it goes.* The references

* The material is drawn from the two great collections of Kennicott (Oxford, 1776–80) and de Rossi (Parma, 1784–88).

enclosed in square brackets are those which, for reasons stated in the footnotes, ought not in my opinion to be counted at all; and accordingly I have not counted them. The last column gives the versional and other support that can be cited for the variant Hebrew reading; and as that column is not likely to be examined except by those familiar with the subject, I need not occupy space in explaining the symbols and abbreviations there employed (see Dahse, p. 52 f.).

At first sight, perhaps, it looks an imposing list. But it will be observed that it is almost wholly made up from Kennicott's collations. Now Kennicott made it his business to register every variant in the MSS. at his disposal, whether good, bad, or indifferent. De Rossi, who had Kennicott's work before him and used it, proceeds on the principle of recording only those readings "quae gravioris aut ullius saltem momenti mihi visae sunt, quae sensum vel mutant, vel afficiunt, et praesidium aliquod habent non modo in MSS. cod. sed etiam in Sam. textu, et in vers. antiquis." Accordingly of the above passages de Rossi considers only seven to be worthy even of mention viz., vii. 1, viii. 15, xvi. 11, xviii. 27, 31, xx. 4. And it will be seen that of the seven three are Adonay-readings, which were peculiarly liable to confusion,

These works were produced at a time when it was hoped that important results for the textual criticism of the Old Testament might accrue from the examination of Hebrew codices. The effect of the publications was to dispel all such expectations. It was found that the variations amongst MSS. were so few and insignificant as scarcely to reward the labour of collation.

and which at any rate in no way affect the literary analysis.

But let us go back to the longer list. It gives 51 variants to 37 names. But of the 51, one (xxxv. 10) is absurd; 11 are omissions which hardly count for anything; 29 are read only by a single MS., 12 by two MSS., 4 by three, and only 6 by four and upwards. Kennicott had collations of nearly 320 MSS. of Genesis in whole or in part (although little more than one-third of these had been *completely* collated). Even if we were to suppose that all the MSS. were fairly accurate a reading supported by certainly less than 4 per cent. of all available codices is not entitled to serious consideration on MS. evidence alone.

Further, it will be noted that of the 6 readings supported by more than 3 MSS. all are Adonay-passages save one (i. 28*b*), and that one an omission. There must be some reason for the preponderance of variants in these cases; and in the last chapter we have seen that the reason is the identical pronunciation of אדני and יהוה as *Adonay*. It is a very instructive proof of the extent to which the MS. variations are caused by clerical errors.

But, once more, it is necessary to consider the *value* of the different MSS., as tested by their general accuracy and by their age. Now of the Kennicott MSS. in the above list, de Rossi affixes a stigma to the following: K9 (thirteenth century: "mendis et rasuris scatet"), 89 (fifteenth century: "multis scatet variationibus, multisque mendis"): and of his own MSS. to the following: R15

(fourteenth century: "scriptus indiligenter"), 18 (thirteenth or fourteenth century: "sed negligentissime scriptus"), 419 (thirteenth century: "sed negligenter admodum exaratus"), 669 (thirteenth century: "scatet tamen omissionibus nec diligenter est scriptus"). K650 we have seen * to be simply a printed edition of the Talmud. I think that all these might fairly be ordered to stand down, as also K103, a fifteenth-century MS. notorious for its accidental omissions. On the other hand, de Rossi gives certificates of excellence to: K4 (twelfth century: "codex bonae notae"), K69 (fifteenth century: "pretiosus codex, etc."), K109 (fourteenth century: "insignis in re critica usus"), K150 (thirteenth century *ex.*: "in hoc solo vel fere solo codice servantur optimae nonnullae var. lect. Samar. T. vel antiquarum vers."), K155 (thirteenth century *ex.*: "melioris notae codex"), K170 (thirteenth century: "codex magni pretii"), K193 (twelfth century: "optim. et antiquus cod."), K248 (thirteenth century: "bonae notae"), K686 (thirteenth century *in.*: "opt. cod. ac sing. . ."), R197 (fourteenth century: "diligentissime scriptus"), R592 (thirteenth century: "singularis in re critica usus. . ."), R469 (fifteenth century: "accuratus, nitidus"), R507 (thirteenth century: "sat diligenter conscriptus"). On the great majority he makes no comment; and we are left to estimate their importance from their probable date. De Rossi (p. xv.) lays down the maxim that for a Hebrew codex to be accounted in any sense old it ought at latest to be of the end of the thirteenth or beginning of

* P. 82.

the fourteenth century. It might be interesting to see how matters would stand if we adopt it as a working rule to strike out of our list (1) all MSS. of whatever age against which De Rossi has placed a bad mark, and (2) all MSS. later than the beginning of the fourteenth century, unless specially certified as good. This would leave the panel of acceptable MSS. somewhat as follows: K4, 69, 80, 109, 150, 155, 157, 170, 185, 191, 193, 199, 227, 229, 239, 248, 252, 384, 601, 686: R16, 197, 245, 248, 251, 266, 296, 412, 464, 507, 592, 754, 766.* If any reader should be at the trouble to revise the list of variants on these lines, he would find that it shrinks to less than two-thirds of its former dimensions; that if we take out omissions and the Adonay-passages (where the literary analysis cannot be affected) there remain but 16 confusions of J, E and JE; that of these 10 † are supported only by one MS., and only one (xxx. 23) by so many as three. What the proportion of chaff to wheat might be in this sifted list we need not try to guess; but even if it were all wheat together (which it certainly is not), I can hardly think that the most aggressive "textual" critic would claim the result as a signal refutation of the pretensions of the documentary theory. When we take into account the general considerations set forth in the preceding pages, we shall hardly be disposed to assign any weight whatever to the indiscriminate citation of variants

* I have now italicized them in Table VI; though it was hardly worth while.

† ii. 18, iii. 23, vi. 5, vii. 9, viii. 15, xix. 29*a*, xx. 11, xxxi. 9, xlv. 5, xlv. 7.

in Hebrew MSS. in which the "textual" critics are wont to indulge.

3. But in justice to my opponents I must now go on to note that they rely not so much on the unsupported evidence of Hebrew MSS. as on the *agreements* of many of their variants with readings found in one or more of the ancient versions. This, they think, is a very strong proof that the readings in question are derived from a Hebrew original independent of the MT. Now in so far as the Samaritan Pentateuch and versions other than the LXX are concerned, the matter will be considered in Chapter V below; and it is enough for the present to point out that corroborations from these quarters are very rare (Sam. 3, Pesh. 1, Vulg. 2), and do not all told amount to a serious challenge to the soundness of the Massoretic text.

But in respect of the LXX, with its plethora of variants, the case is naturally different. If, indeed, we take only those readings which are supported by the bulk of LXX authority, we find that there are only two or at most three cases to consider (i. 28*a*, xix. 29*a*, iii. 22?)—a negligible quantity. It is of course admitted that in these cases it is a question whether the LXX, backed by Hebrew MSS., may not have the original text; but they are so few that even if in each case the MT should happen to be wrong its general authority as against isolated MS. divergences would not be impugned. But if we are to reckon up all the instances where a Hebrew variant has *some* support from LXX MSS. or daughter-versions or citations, no doubt the

number is considerable. I have noted in the last column of Table VI the LXX evidence for the various readings—not very carefully, but mostly trusting to the statistics furnished by Dahse. It will be seen that 22 Hebrew variants agree with some form of LXX text. But here the so-called "textual" critics seem to blunder egregiously. They argue that even a solitary Greek MS. acquires importance, as indicating an original Hebrew text, if it be in agreement with a single Hebrew MS.; and of course *a fortiori* if there be two or three on either side. To reason thus is to perpetrate a gross mathematical fallacy. The doctrine of probabilities comes into play. Our opponents overlook the fact that the limits of possible error are extremely narrow, while the chances that an accidental error in a Hebrew MS. will coincide with a reading in the apparatus of the LXX are remarkably good. That is to say, if a Hebrew scribe went astray from the MT in copying a divine name, he could only substitute E for J or J for E (in rare cases a JE might afford a wider choice of error); and in either event he would be pretty sure to find his mistake "confirmed" by some MS. of the LXX. I calculate roughly that in about two-fifths of the names contained in Dahse's tables *both* the alternative readings occur in LXX MSS. or daughter-versions, or citations; so that if a Hebrew MS. differs from the MT it has two good chances in five of finding some kind of support in the LXX. In all but two (i. 28, xix. 29*a*) of the 22 actual instances of agreement between Hebrew and Greek MSS. the Massoretic reading is also represented in MSS.

of the LXX, and in the vast majority of cases far more strongly attested than the variant. In these circumstances it is mere pretence to speak of coincidence as corroboration, or to argue that a variant derives importance from the fact of its occurring in two unrelated series of documents. We can now measure the importance of Dahse's assertion (p. 51), "Die Varianten der LXX werden geschützt (a) *durch hebr. MSS.*"*

* I would here call attention to v. 22, which sheds a lurid light on the value of Hebrew "corroborations," and also on the incredible perfunctoriness with which such variants are cited by writers like Wiener and Dahse. (See Table VI.) The facts are these: (1) K151 omits the entire verse: I presume that Dahse will not defend *that* text. But his statement that "K151 stimmt mit E" is wholly erroneous. E (a Greek uncial) simply substitutes *καὶ ἔζησεν Ενὼχ μετὰ κτλ.* for *εὐηρέστησεν δὲ Ενὼχ τῷ θεῷ μετὰ κτλ.*, in conformity with the stereotyped formula used throughout the chapter. If Dahse should maintain that this is the original text, I should not object; but that is neither here nor there: it is *not* the text of K151. (2) The Greek cursives *HP* 73, 74, 134 (= t) read practically as E (*ἔζησε δέ*), and to cite them (as Dahse does) as simply omitting *τῷ θεῷ* is thoroughly inaccurate and misleading. (3) K191 omits את־האלהים, yielding the impossible sentence, "And Enoch walked after he begat, etc." The only LXX MS. that *appears* to confirm this nonsensical reading is *HP* 79, which has *εὐηρέστησε δὲ Ενὼχ μετὰ κτλ.*, "And Enoch pleased after he begat, etc.," which is just as absurd as the text of K191. But (*a*) it is to be observed that *εὐηρέστησε δέ* corresponds not to the bare ויתהלך but to ויתהלך את־, so that it does not agree with K191. (*b*) The agreement is not merely superficial, but clearly accidental. At least it is presumable that the peculiar reading of 79 was brought about by a secondary correction of the *καὶ ἔζησεν* of E to the *εὐηρέστησε δέ* of the ordinary LXX, the copyist not perceiving that he was making nonsense of the verse by

4. Lastly, it is alleged by textual critics that there are passages where the MT is *on internal grounds* "demonstrably" wrong in its use of the divine names, and where the true reading has been preserved in a small minority of Hebrew or Greek MSS. I reply that I do not believe any such case exists, and that certainly none of those that have been adduced will be found on examination to bear out the contention.

The passage most persistently cited in this connexion is Genesis xvi. 11. From the time of de Rossi at all events it has passed from hand to hand as a palpable proof that the MT cannot have preserved the original name. Dahse, supporting Wiener's philippic against the present writer, says (p. 32): "mit Recht . . . macht Wiener mit allem Nachdruck geltend, dass nicht bloss, wie Skinner es tue, die 49 Fälle der Abweichungen, die Redpath und Eerdmans nach Swete anführen, zu berücksichtigen seien, sondern auch zahlreiche (?) andere Stellen, wie z.B. Genesis 16[11], wo die Handschriften **bw** (mit OL, arabs uterque, und hebr. MS.) *offensichtlich* mit ihrem ὁ θεός das Richtige böten." Let us then consider the import of xvi. 11. It gives an etymology of the name Yishma'-el (ישמעאל="may *El* hear") in the words "for *Yahwe* has heard, etc." (כי שמע יהוה וגו׳). This, we are told, is a glaring and impossible contradiction. Wiener, with characteristic presumption, says that the name Yishma'-el *must* have been explained by a sentence containing Elohim, for if the explanation had contained the name

overlooking the τῷ θεῷ which followed. Many MSS. exhibit conflation of the two readings.

Yahwe the name *must* have been Yishma'-yah! He seems to imagine that Ishmael is a fictitious name, whose form could be changed according to the taste and fancy of the speller. In reality it is the historic name of a tribe which no writer could alter from merely literary motives. That is a confusion of ideas which is extraordinary even in a mind untrained to exact philological thinking; and I have not observed that any other writer has put the matter quite so crudely. But they all alike labour under the illusion that *El* and *Elohim* are convertible terms. It is a wonder that none of them have thought of taking up a hint of the cautious de Rossi, who, after defending Elohim as "conformior" to the name Ishmael, says "huic affinis ac congruentior est lectio cod. mei 754 ex prima manu כי ישמע אל עניך"; although he adds "Sed Jeoah ipsa, ut videtur, primi scriptoris manu ad marginem restitutum est." We must suppose that there was a time when the interpretation of such a name as Ishmael would have been expressed in a sentence like "*El* hears"; and the courage of our textual critics might well have proved equal to the advocacy of the claim of R754 to be the sole representative of that primitive etymology. However, they have not done so; and we have simply to insist, against their contention, that El is no more Elohim than it is Yahwe. It is an archaic name for the Deity which had ceased to form part of the ordinary spoken language* before these narratives were reduced to writing, and which had to be replaced by one of the two names for God current in

* For details, see Driver, *Genesis*, p. 403.

common speech. There is absolutely no reason except usage why one of them should be used in preference to the other. If a writer habitually used Yahwe he would naturally say כי שמע יהוה; just as readily as another who habitually used Elohim would write כי שמע אלהים. The latter phrase actually appears in the parallel passage xxi. 17, where the Elohist is giving *his* etymology of the name Ishmael. So that instead of xvi. 11 weakening the evidence for the documentary theory, it furnishes in reality one of its most striking detailed confirmations.*

The case is on all fours with the explanation of the name Samuel (שמואל) in 1 Sam. i. 20, where the MT has כי מיהוה שאלתיו: "for from *Yahwe* I asked him." This reading is supported by all Hebrew MSS., by the Peshitta, the Vulgate, and even the LXX. For although a good many MSS. vary from the chief printed editions (Swete, after A, B: *παρὰ Κυρίου θεοῦ σαβαώθ*), there is not one which omits the *κυρίου*.† Will the "textual"

* The other 10 cases of "demonstrable" inferiority adduced by Wiener (*Essays*, pp. 16–19) are unworthy of serious notice, except xiv. 22, where a *combination* of external and internal evidence makes it probable that *Yahwe* is a gloss. In xxxi. 42, 53, and probably also in xlviii. 15, *God* is used appellatively, and has nothing to do with our problem. With xxx. 24, 27, I have dealt above (p. 54 f.). How any man could have the assurance to adduce either these two passages or iv. 1, 26, xv. 2, xxviii. 13, as cases where MT is demonstrably wrong on internal grounds, is to me incomprehensible. If there be a case where MT is demonstrably *right*, I should say it is iv. 26.

† The principal variants are the following: *Κυρίου σαβαώθ* (9 MSS.); *Κυρίου σαβ. παντοκράτορος* (4 MSS.); *Κυρίου σαβ.*

critics maintain, in the teeth of all textual evidence, that Yahwe is there an impossible reading, and must have been substituted for Elohim? If not, their argument in the parallel instance of Ishmael completely breaks down. And if they do, they so stultify their own method that we need no longer give heed to their opinion. At all events, I hope we shall hear no more of Genesis xvi. 11 as an instance where the MT is "demonstrably" wrong.*

A very similar, but even weaker, instance cited by Dahse is the etymology of Reuben in xxix. 32 (p. 44). He says it is "allbekannt" that the name was originally *Rubel*; whence it follows that Yahwe in xxix. 32 ("Yahwe has seen my affliction") cannot be original. Well, one would like to hear what *was* original there. Dahse does not even tell us how he understands the name ראובל: he rather gives the impression that he holds the utterly impossible view that it is a compound of ראוב and אל. I will assume, however, that he takes its second component to be בל=בעל (Baal), used as a generic title of the Deity; and that its signification is "seen of Baal" (see *ICC.* p. 386).† Supposing that to be the correct name, and the etymology intended, we must again assume that in early times the interpretation was expressed

$\theta\epsilon o\tilde{\upsilon}$ $\pi\alpha\nu\tau$. (3 MSS.); Κυρίου Κυρίου $\sigma\alpha\beta$. $\pi\alpha\nu\tau$. (1 MS.); Κυρίου alone (1 MS.). Κυρίου $\pi\alpha\nu\tau$. (Complutensian Polyglot).—$\pi\alpha\nu\tau o\kappa\rho\acute{\alpha}\tau\omega\rho$ is the usual rendering in the LXX of צבאות (=$\sigma\alpha\beta\alpha\acute{\omega}\theta$), "Hosts."

* See NOTE VII, p. 275.

† The ordinary explanation supports the form ראובן: רָאוּבֵן=רָאָה בְעָנְיִי, a mere verbal assonance.

in that form, like the explanation of "Jerubbaal" in Jud. vi. 32. Now when Baal ceased to be used as an epithet of the national God and was appropriated to a strange deity, the name could be retained in Jud. vi. 32, because it implied in that connexion no recognition of the false god. But in Genesis xxix. 32 its retention would have attributed heathenish worship to the patriarchal family, and it was necessary to replace it either by Yahwe or by Elohim. And when it was exchanged for a name of the true God, it was just as legitimate and natural to replace it by Yahwe as by Elohim. There is therefore not the slightest *internal* ground for questioning the correctness of the Massoretic reading.

Amongst the passages where Dahse tries to show that the Yahwe of the MT cannot possibly be right for internal reasons, there are two which he thinks are proved to have been originally Elohim-passages by independent tradition or by allusions in Hebrew literature. The first is the account of Jacob's wrestling at Peniel (Genesis xxxii. 24 ff.). This, he says, is known to be attributed to J. That is not quite so. It has also been attributed to E; and the drift of recent criticism has been to regard it as a composite narrative in which J and E have been amalgamated (see *ICC.*, p. 407). But however that may be, it is certain that the only divine name which occurs in the MT is Elohim (*vv.* 29, 31). What then is the sense of citing Hos. xii. 4 (with Elohim) and arguing that if Hosea had read it in a Yahwistic book he would certainly not have used Elohim here? We do not even know that Hosea

read it in any book; if he did, there is no reason why it should not have been an Elohistic book; *but* even if his written source was our present Yahwistic document, he would surely use the divine name which occurs in that document, which, as we have seen, is Elohim. The argument thus turns round into a singular confirmation of the scrupulous accuracy of the tradition of the divine names in the Hebrew text. And Dahse might at least have mentioned that the critics who assign the passage to J are guided by something else than a slavish regard to the divine names of the MT. The second example is the story of the overthrow of Sodom and Gomorrah in Genesis xix. (cf. xiii. 10). According to the MT this is a Yahwistic passage, and is therefore assigned by critics to J. But in three prophetic passages (Am. iv. 11, Isa. xiii. 19, Jer. l. 40) it is referred to as a destruction wrought by *Elohim*. Now, to begin with, the force of these three passages is neutralized by Deut. xxix. 33 and Ezek. xvi. 48 ff., where Yahwe is used. But the root of Dahse's error lies deeper. "If there was a version of the story which favoured a particular designation of the Deity . . . it must have been an Elohistic narrative, and not the Yahwistic which we now find in the MT" (p. 42). No one denies that the oldest version of the story may have been Elohistic: indeed the whole point of the contention that it was so is that it was a foreign myth imported into Israel, in which the name Yahwe could not possibly have been used. But that is not the question that Dahse has to face. The question is not of how the narrative

read in its primitive form, but of the literary shape into which it was cast by the author of the account we have in Genesis. If for the true God who was the author of the catastrophe that writer habitually used the name Yahwe, why should he not do so in this instance as in every other? As for the prophetic allusions, Amos lived at a time when the primitive form of the myth may well have been a living memory, and there is no reason to suppose that he had no authority other than our present Yahwistic document. And if late prophets like the authors of Isaiah xiii. 19 and Jeremiah l. 40 still continued to use Elohim in connexion with this incident, that does not mean that they read Elohim in the Genesis narrative. It is much more probable —it is, indeed, all but certain—that they had in their minds the unusual phrase of Amos,* which they reproduce *verbatim et literatim*. Again the Massoretic reading in Genesis comes unscathed out of the text-critical ordeal; and the occasional Elohims of the LXX have no greater probability than they acquire from purely textual evidence (here sufficiently slender), as in all other cases.

### 2. *The Samaritan Pentateuch.*

The Samaritan Pentateuch is a recension in Hebrew of the books of Moses, as used by the schismatic community whose religious centre was and still is the temple on Mount Gerizim at

* במהפכת אלהים את סדם ואת עמרה, where the את shows that the noun מה has the force of an infinitive.

Shechem. Its MSS. are written in the Samaritan script—a degraded form of the original Hebrew alphabet; but otherwise they are simply a special group of Hebrew codices, and are in fact often treated as such by writers on the Old Testament text. The characteristic features of the recension appear in a series of intentional alterations of the parent text, due to editorial motives and tendencies. The most striking of these are, of course, the few changes introduced in defence of the legitimacy of the Samaritan temple and worship, such as the command to build an altar on Mount Gerizim after Exodus xx. 17, Deuteronomy v. 18, and the substitution (or retention) of Gerizim for Ebal in Deuteronomy xxvii. 4.* Many alterations spring from the desire to produce a smoother, more intelligible and more consistent text: archaic or abnormal grammatical forms are eliminated, discords of gender and number are avoided, exegetical difficulties are removed by glosses and emendations, and inconsistencies are reconciled by corrections or short interpolations from other contexts. Besides these there are a number of lengthy insertions from parallel passages, which form one of the outstanding peculiarities of the Samaritan text. Some of these alterations are clearly of Samaritan origin; but in other cases it must remain uncertain whether they are the work of Samaritan editors or were found in the Jewish MSS. on which the Samaritan Pentateuch is based. Taken together, however, they impart a distinctive complexion to the Samaritan text, which is

* It is immaterial to the present discussion whether the Jews or the Samaritans have here kept the original text.

recognizable as far back as we can trace its history.

If now we set aside these characteristic differences, we find in Samaritan MSS. a consonantal text which very closely resembles the MT. The variants are for the most part of the same kind and the same order of magnitude as those found in extant Hebrew MSS., and the majority of them scarcely exceed in importance those which seem to have existed in Jewish MSS. of the Talmudic age. The interesting and perplexing fact is that where the Sam. does vary from the MT it often agrees with the LXX. The LXX never supports the Sam. in its polemical variations in defence of the Samaritan cult; nor does it reproduce the long harmonizing supplements referred to above. But in minor interpolations, in glosses and emendations, as well as in many readings not due to any tendency, it often follows the Sam. The agreements are often in minutiae, and acquire importance from their very minuteness; but in certain cases the combined LXX and Sam. text is on internal grounds to be preferred to the MT as the superior and, therefore, the original reading.

These relations of the Sam. to the MT on the one hand and to the LXX on the other, constitute a textual problem of great intricacy, and have given rise to the most diverse theories of the probable connexion between the Samaritan and Greek recensions. Some scholars have gone so far as to maintain the opinion, now generally recognized as untenable, that the LXX is a direct translation from the Sam.; while others have been driven to the most complicated hypotheses of

correction and revision of one text by the others in order to account for the phenomena which confront us here. To enter upon such questions is quite beyond the scope of this article. In what follows I will adopt the view which seems most plausible in itself, and at the same time is most favourable to those who would use the Samaritan Pentateuch as a witness along with the LXX against the MT: viz., the theory of Richard Simon and Gesenius,* that Sam. and LXX are both derived from a group of Hebrew MSS. containing a text which, while not quite homogeneous, differed as a whole (and occasionally for the better) from the official Jewish recension which is the parent of our Massoretic text.†

* A good account of Gesenius's essay and arguments may be read in the art. "Samaritan Pentateuch," by Emmanuel Deutsch, in Smith's *Dict. of the Bible*, vol. iii.

† Before entering on the somewhat involved discussion that lies before us, it is right to point out that a critical edition of the Samaritan Pentateuch does not as yet exist. It is understood that two are in contemplation: one by the English Text and Translation Society, and the other in Germany under the editorship of von Gall. It may be expected that when such an edition appears, its prolegomena will shed new light on some of the problems which at present beset the path of the inquirer. It is not improbable that a critically revised text will remove some uncertainties which cannot now be cleared up; and it is extremely probable that it will at least provide data for a more precise determination of the affinities between the LXX and the Sam. than we can yet formulate. We must therefore be prepared to find that the best solution of the problem that we can compass with our imperfect material may be upset or modified by the improved critical apparatus which will one day be at our disposal. But unfortunately we cannot wait for this. We must make the best of an obscure situation,

In the light of these facts, we proceed to consider the evidence of the Samaritan Pentateuch as bearing on the transmission of the divine names. We have already seen (p. 38) to how slight an extent it differs from the MT in this respect in the book of Genesis. There are only at most nine passages where it has a different reading, viz., vii. 1, 9, xiv. 22, xx. 18, xxviii. 4, xxxi. 7, 9, 16*a*, xxxv. 9*b*, the last being an Elohim supplied where MT has no name at all. We have also found that only three of these variants (vii. 1, 9, xxxi. 9) appear in Hebrew MSS., and we shall see later that only two (vii. 1, 9) find support in any of the younger versions (Pesh., Vulg.) How, then, does the case stand as between Sam. and LXX? In five out of its nine divergences from MT (xiv. 22, xxviii. 4,

and be satisfied if we can exhaust all the reasonable probabilities that come within our view. The text that we have mainly to rely upon is that of Walton's Polyglot Bible, reprinted separately by Blayney in Hebrew square characters in 1790. This is anything but a critical edition. It is merely the text of the Paris Polyglot of 1645, freed from typographical errors; and that again was carelessly edited by Morinus from the first MS. of the Samaritan Pentateuch that came under the eyes of European scholars. Kennicott collated the text of the London Polyglot with 16 other MSS., whose readings of the divine names are identical with those of the printed edition, except in four places (ii. 5, iii. 14, xxvi. 24, xxxi. 16) where a Yahwe or Elohim is omitted by one or other of two MSS. (61 or 64). In 1868 Petermann published a list of variants from MT of the famous Torah-roll of the sanctuary at Nabulus, in which one Sam. variant to a divine name (Genesis vii. 1) does not appear. These are our accessible sources of information as to the text, and it is evident that for the purpose in hand it is needless to go beyond the London Polyglot.

xxxi. 7, 9, 16*a*) the Sam. is unsupported by a single MS., citation, or daughter-version, of the LXX. In three (vii. 1, 9, xx. 18) it agrees with a slenderly represented Greek text.* Only in one passage is the Sam. backed against MT by the united authority of the LXX : viz., the addition of Elohim in xxxv. 9*b*; a difference by which the documentary analysis is in no way affected. Now it seems to me that in dealing with this matter our text-critical opponents show a singularly ill-balanced judgment. When they light upon a discrepancy between Sam. and MT they rejoice like men who find great spoil; but an agreement between these two appears to be in their eyes of no significance. Especially are they delighted when they discover a coincidence of the Sam. with something else against the MT. When Dahse can point to a solitary correspondence in a divine name between one of his Greek recensions (**egj**) and the Sam., it is an "auffallende Erscheinung" (p. 106); but the 310 (or so) correspondences of Sam. and MT are not deemed worthy of mention. I have explained above (p. 103) why I refuse to consider an isolated agreement between a Hebrew MS. and some form of the LXX as in the least surprising; and the same argument holds good as applied to a casual agreement of the Sam. with some form of the LXX. On the other hand, the agreement of the Sam. with the MT in more than 300 cases *is* a remarkable phenomenon—perhaps, all things considered, the most remark-

* The details, as given in the apparatus of the Cambridge LXX, are : vii. 1, Sam. supported by **cw** Arm.-codd. ; vii. 9 by E ; xx. 18 by **bw ej** Boh., Phil.-Arm.

able phenomenon in the history of the Hebrew text. We shall see presently what that means. In the meantime let us be clear on this point, that just so far as a discrepancy between Sam. and MT throws doubt on the correctness of the latter, precisely to that extent does a correspondence of the two guarantee the soundness of the MT. And since there are over 300 agreements in the divine names to 9 differences, it is surely well within the mark to say that the Samaritan recension as a whole is on the side of the MT and immensely strengthens its claim on our confidence.

It would really be difficult to exaggerate the importance of this fact. It means that through two independent lines of descent the divine names in Genesis have been transmitted with practically no variation. That, in the first place, is a very strong confirmation of the view several times expressed in the course of this volume, that Jews as well as Samaritans exercised the most scrupulous care in the transcription of the name of God. But it implies, further, that at the time when the two texts became independent of one another, the distribution of the divine names represented in each was already established. We may not assume that other distributions were not in existence in MSS. of that period; but we are sure that at all events *this* distribution must have been represented in MSS. of sufficient authority to be accepted as the basis of the two most important recensions of the Hebrew Pentateuch. The question now is, how far back does the point of divergence lie? In other words, what is the age

of the Samaritan Pentateuch as a separate Law-book, distinct from the Jewish Pentateuch from which our present Massoretic text has descended? Since this is a matter on which some difference of opinion obtains, it may be well to state pretty fully the grounds for determining this date.

Amongst modern critical historians the prevalent view has been, and probably still is, that the Samaritans received the Pentateuch from the Jews in the time of Nehemiah, about 430 B.C. It would be extremely convenient for my present argument to accept that date; for it would carry back the testimony of the Samaritan Pentateuch to within a short interval of time from the final redaction and promulgation of the Law by Ezra (at the earliest, *c.* 444 B.C.). But it seems to me that the evidence points to a date about a century later. The conclusion of the critical historians is reached by combining a brief enigmatic notice in the book of Nehemiah with a circumstantial narrative found in the eleventh book of the *Antiquities* of Josephus (§§ 302–324). In Nehemiah xiii. 28 we read of the expulsion from Jerusalem of a grandson of the High Priest Eliashib, because of his marriage with a daughter of Sanballat the Horonite. Josephus also tells us of a priest (named Manasse), who was a *great*-grandson of Eliashib, and who was excluded from the succession to the high-priesthood because he refused to be separated from his wife, a daughter of Sanballat the governor of Samaria. Thus far it certainly looks as if we had here two versions

of the same story. But Josephus goes on to relate how Manasse seceded from the Jews, on the promise of Sanballat that he should be High Priest of the Samaritans, and that a temple should be built for him on Mount Gerizim, as soon as the permission of Darius, the last Persian monarch, could be procured. He adds the important statement that many disaffected priests and Levites in Jerusalem joined Manasse in his secession. In the meantime the Persian Empire was overthrown by Alexander the Great; and it was he who granted the firman under which the temple was erected on Gerizim, and Manasse was installed as High Priest (*c.* 330 B.C.). The common opinion, then, has been that the notice of Nehemiah xiii. 28 is to be supplemented by the account of Josephus; so that the building of the schismatic temple, the definite organization of the Samaritan sect, and the establishment of a regular priesthood and cultus at Shechem are to be assigned to about 430 B.C., instead of a century later, as Josephus states. But this is obviously a very unsafe combination. Whatever may have taken place under Nehemiah, the names of Darius and Alexander are too closely and explicitly associated by Josephus with the building of the temple to be set aside as unchronological. While there may have been some confusion in the mind of that writer with events of Nehemiah's time, we have no right to transfer his narrative bodily to a date 100 years earlier; and it is reasonable to allow that he was probably well informed in assigning the building of the Samaritan

temple to the early reign of Alexander. If so, we must infer that the final constitution of the Samaritan community on a religious basis, with priesthood and ritual, belongs to that time.

It will be observed that neither Nehemiah nor Josephus says anything about the adoption of the Pentateuch by the Samaritans. We are thrown back, therefore, on general considerations to decide whether its introduction is more probable at the earlier or the later date. Such considerations seem to point clearly to the latter alternative. For one thing, a Law-book such as the Samaritan Pentateuch is would have been a useless and inconvenient possession to the Samaritans in the absence of a Levitical priesthood and a regular sanctuary, and the latter at least they had not secured before 330. Another consideration, which must weigh with those who hold the critical theory of the Pentateuch, is that the redaction of the Law-book cannot be put earlier than the year 444; and a much longer time than fourteen years must be allowed for the rise of such variations of text as appear in the Jewish and Samaritan recensions. The time of Alexander, on the other hand, is the latest period to which the adoption of the Pentateuch by the Samaritans can with any plausibility be assigned. For if a separate Mosaic Law-book would have been an encumbrance to the Samaritans before the building of their temple, it would have been impossible for them *after* that event to maintain their sectarian position without a Law-book adapted to their separatist pretensions. This view appears to be gaining ground amongst the most recent writers

on the subject, and it would be easy to quote the opinion of several scholars in favour of it. We will therefore accept the year 330 B.C. as the proximate date when the Pentateuch passed into the possession of the Samaritan community.

If this conclusion be correct, the Samaritan Pentateuch is still the oldest external witness we have to the state of the early Hebrew text. It takes us back to a date within a century of the final redaction of the Law. There is no doubt whatever that even at that early period errors had crept into the Hebrew text. Where the Sam. and MT agree (as they not infrequently do) in a reading which is manifestly corrupt, we are sure that that corruption had taken place before the two texts had parted company, i.e., by about the middle of the fourth century.* It is, therefore, conceivable that mistakes had occurred in the transmission of the divine names before that time. What we know for certain is that the common element of the two texts, which includes the divine names in Genesis, goes back to the fourth century, and that from that time the exact distribution of the names which then obtained has been preserved with all but complete fidelity in two independent series of MSS. down to the fifteenth Christian century or later. And this, I repeat, is amongst the oldest facts that textual criticism has established in the history of the text. The LXX, which

* For while such frequent causes of error as confusion of ד and ר or ה and ח might occasionally produce an identical corruption in two texts independently, accidental coincidences of this kind cannot be numerous; and there are cases of a more complex character where agreement in error cannot be ascribed to chance.

is our next oldest witness, only furnishes evidence of the state of things about the middle of the third century, when the Greek translation of the Pentateuch was made. When we consider how, in regard to the names for God, the value of the LXX is depreciated by the notorious confusion of its MSS., and the consequent impossibility of ascertaining (in many cases) its original readings, we cannot hesitate to assign a decisive importance for our present inquiry to the critical fact presented by the agreement of the Samaritan Pentateuch with the Massoretic text.

Thus far, however, I have argued on certain current assumptions which until quite recently hardly any one has ever thought of calling in question. The argument assumes for one thing that the Jewish and Samaritan Pentateuchs have been uninfluenced by each other from the day when they first parted company. It assumes further that the LXX, even in cases where its peculiar readings undoubtedly represent a Hebrew original, only yields information as to the text contemporary with the translation, i.e., for a text at least half a century younger than that which we reach by comparison of the Sam. and MT. It is necessary now, in view of some recent discussions, to test the validity of these and other assumptions. For it is not to be supposed that an experienced critic like Dahse has failed to consider the serious and indeed insurmountable objections which the ordinary view of the history of the text presents to his attitude towards the text in general, and in particular to several of his speculative constructions which have been dealt

with in these articles; such as the elimination of *El Shaddai* in Exodus vi. 3 and elsewhere, the Sedarim-hypothesis in more than one aspect, and the theory of Hebrew recensions older than the MT. All these make shipwreck more or less completely on the fact of the Samaritan Pentateuch as usually understood. He must, therefore, have formed some conception of the relation of Sam. to MT which to his own mind justifies the very slight and unbalanced regard which he pays to its testimony. We may expect that in some future volume of his textual studies he will take his readers into his confidence, and tell them what he really thinks on this important matter. He has not done so as yet; and we are left to conjecture what his position is likely to be. So far as I can see there are just two courses open to him. He must either (1) believe that the *present* text of the Sam. has broken off from the Jewish stem at a much later point than the first adoption of a Law-book by the Samaritans, and later also than the LXX, or (2) accepting the common opinion as to the ages of the Sam. and the LXX, he must hold that the *Hebrew original* of the LXX is of greater antiquity than the Samaritan Pentateuch, although the translation into Greek was not made till the time usually supposed. Both these theories have been actually put forward, and neither can be pronounced *prima facie* impossible. We must consider briefly how far they are defensible in themselves, and how far they affect the conclusions arrived at above.*

* It will be said that there is a third possibility: viz., a progressive assimilation of the Sam. text to the later Jewish,

1. The first view has been advanced in a very definite form by Professor Kennett in the *Cambridge Biblical Studies* (1909, p. 126). He thinks it probable that all copies of the Samaritan scriptures were destroyed by the Jews at the capture of Samaria and the destruction of the Gerizim temple under John Hyrcanus (*c.* 107 B.C.), and that the Samaritans restored their law on the basis of Jewish MSS. of contemporary age. Thus the witness of the Sam. to the text of the Old Testament, instead of going back to the fourth century, goes back at the earliest to the last quarter of the second century. There is no doubt that that theory would invalidate most of the conclusions which we have drawn from the existence of the Samaritan Pentateuch. Is it an admissible theory? It seems to me improbable in a very high degree. It is admitted that the Samaritans possessed a Pentateuch from the time of Nehemiah. Now the entire destruction of a sacred book is at all times a difficult operation. In the case supposed it is an unlikely result of the conquest, and the replacement of it by a new Jewish Law-book is in the circumstances more unlikely still. We can hardly think that at a time of such hatred and resentment the Samaritans would have easily reconciled themselves to the

due to the friendly relations subsisting between the heads of the two communities. Of that position I can only say that in the first place it is too nebulous to admit of discussion; and in the second place that it seems so improbable that it could only be accepted as a last resource to relieve a problem otherwise insoluble. Either of the alternatives mentioned above would be preferable.

adoption of a new code from their conquerors. They would rather have fallen back on the most imperfect copy of their own scriptures than have started afresh from ground common to themselves and their foes. Nor can we readily suppose that no such copy was to be found. We must bear in mind that long before then there was a numerous Samaritan diaspora in Egypt, if not in other lands; and even if the Jews had succeeded in destroying all MSS. of the Pentateuch in Samaria itself, it must surely have been possible to procure one from the Egyptian colonists. These are only conjectures, but the theory itself is a conjecture; and I cannot think that the balance of evidence is in its favour.

2. The second theory demands a somewhat fuller examination, because it is more plausible in itself, and because it opens up questions which go to the roots of the textual problem of the Old Testament. It is that while the Samaritan Pentateuch is older than the Greek translation of the Law, yet the Hebrew original of the LXX had broken away from the Jewish line of transmission at an earlier period than the Sam. To put it more definitely, the LXX was translated from the last of a line of Hebrew MSS. which had had an independent circulation in Palestine or Egypt from a time anterior to the separation of the Samaritan text from the Jewish. This view has been advocated with considerable ability and unusual moderation of statement by Wiener in the *Expositor* of September, 1911; and if it were necessary to try conclusions with that irascible controversialist, it would not be difficult to show

that with all his logical acumen he has contributed nothing of value to the elucidation of the question he has raised.* But it is unnecessary to do that

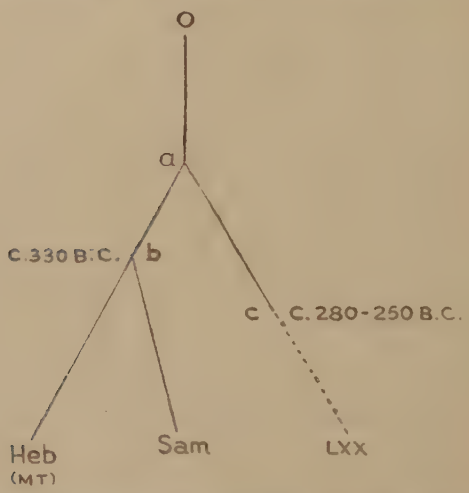
* The article, it may be explained, is mainly devoted to a demonstration, on the narrow and inadequate basis of four short passages taken at random from the Pentateuch, that the Hebrew original of the LXX differed far more widely from MT and Sam. than these differ from one another. I cannot here discuss the variants *seriatim;* but after careful examination it is clear to me that their significance is constantly misunderstood and greatly exaggerated. I have found very few which really point to a different Hebrew from MT. Apart from cases of inner-Greek corruption, the great bulk of the differences registered are clearly due to the freedom of translators, who did not render word for word (after the manner of Aquila), but often adjusted their rendering to Greek idiom, assimilated expressions and grammatical forms to the context, substituted synonyms, and made slight explanatory additions. Moreover, in the few cases where a divergent Hebrew may reasonably be suspected, there is hardly one in which that Hebrew commends itself as superior to the MT; while there are several where it is distinctly worse; and this is true alike as regards the consonantal text and the vocalization. Wiener overlooks the crucial consideration that only where the Hebrew basis of the LXX is obviously better than the Hebraeo-Samaritan does it count as evidence that the former is nearer to the original text than the latter. For if the original of the LXX is obviously inferior to the Hebrew, then, while it is certain that the corruption is of more recent date than the separation of the LXX Hebrew from the parent stem, it must always remain doubtful in such cases whether that separation was earlier or later than the severance of Sam. from MT. Hence, on my reading of the facts, Wiener's very restricted argument has failed to establish the thesis that the LXX has a higher Hebrew ancestry than the Sam. Of course I am aware that if even a few superior readings in the LXX can be produced, they are sufficient to prove that it depends in part on an older Hebrew than Sam.; on the

in detail. The whole argument moves within this syllogism: (*a*) The agreement of Sam. with MT is greater than that of LXX with either; now (*b*) the more divergent text is the more ancient; therefore (*c*) LXX represents an earlier phase of Hebrew text than Sam. or MT. The minor premiss (*a*) will, I believe, be generally conceded; but the major (*b*) only with the qualification that everything depends on the nature of the divergence; and hence the conclusion (*c*) remains in suspense.

It is necessary to look very carefully at the presuppositions of this argument, because there are several points at which error is apt to creep in through imperfect realization of the facts of the case. It can be best stated by attending to examples of *corrupt text* in the various recensions. The MT contains a few undoubted corruptions which are not in the Sam., and the Sam. contains a greater number from which MT is free. These must be assumed to have crept into either text *after* the time when they went their separate ways; and on the other hand corruptions which appear in both must have come in *before* that time. But in the same way, the joint MT and Sam. text has a few errors which the LXX does not share, just as the LXX contains a number of undoubted corruptions which are not found in the Heb.-Sam.; and again we infer that both

other hand the undoubted presence of inferior readings in its Hebrew basis means that we cannot predicate a higher antiquity of its text as a whole, or assume that in neutral readings (i.e., those to which no intrinsic test of value can be applied) the presumption of antiquity is in its favour.— See NOTE VIII, p. 276.

these classes of errors are later than the divergence of the Hebrew basis of the LXX from the parent stock of the MT and Sam. But that obviously means that the Hebrew basis of the LXX has branched off from the common stem at a point nearer the origin than the bifurcation of Sam. and MT. The assumed situation, then,




may be represented graphically by the accompanying diagram; where the continuous lines denote transmission in Hebrew MSS., and the dotted line the succession of Greek MSS. O stands for the common Hebrew original of all the texts, *a* for the problematical point of separation of the LXX from the Jewish texts; *b* for the separation of MT and Sam.; and *c* for the point at which the Greek translation was made.

Now, to the principle involved in this abstract reasoning it does not seem to me that any ex-

ception can be taken. But unless I am greatly mistaken, Wiener goes astray through over-simplification of the very complex phenomena we have to deal with. Divergences of the LXX from the joint Sam. and Jewish text may be of four kinds: (1) They may be readings of the original text which have been lost in the Heb.-Sam., but preserved by the LXX; i.e., they may go back to the line O—*a* in the diagram. Where the LXX has clearly the better text we must assume that its reading falls under this head. (2) They may have originated in Heb. MSS. before the Greek translation was made (in the line *a*—*c*). (3) They may have come in in the translation itself (at the point *c*), through actual errors, misreading of the Hebrew, non-literal renderings, explanatory additions, and so on. (4) In the subsequent history of the Greek text of the LXX (below *c*). I have pointed out in the last note that in my view Wiener makes no sufficient allowance for changes of the class (3). I have now to add that he does not seem to recognize at all the existence of class (2). He appears to fancy that when he has proved that a reading of the LXX rests on a Hebrew variant, he has recovered a text which goes back to the point *a*, where the Hebrew of the LXX parted from the other line of descent; and thus identifies the text of a Hebrew MS. of say 250 B.C. with the text common to all MSS. at the unknown period when the LXX is believed to have broken away from the joint transmission. Whereas it is only where the LXX has preserved the intrinsically *superior* text that such inferences are legitimate.

Perhaps the fallacy lies in thinking of the LXX as representing an *older text* than Sam., because its Hebrew basis started on its separate course at an earlier time. It is surely superfluous to remark that the Heb.-Sam. diverged from LXX just at the same time as LXX diverged from Heb.-Sam.; and that it is sheer inconsequence to think of the one text as older than the other on this account. It would be nearer the purpose to consider that, properly speaking, all texts are of equal antiquity; that is to say, if we have three contemporary MSS., a Greek, a Samaritan, and a Jewish, the presumption is that each will have been the result of as many successive copyings as the others, and that consequently all have been equally liable to the accidents of transcription. As all living men trace their ancestry to Adam, so all MSS. of the Pentateuch descend alike from the edition of the Law promulgated in the fifth century; and the fact that the hypothetical original of the LXX dates from an earlier period than the original of the Sam. no more guarantees the superiority of the *actual* text of the LXX to the text of Sam. than a man's having an unknown ancestor in the time of the Conqueror would give him precedence over another man who had a known ancestor in the reign of Charles II. There is, in fact, no question of relative antiquity, but only of relative soundness, of text; and that can only be determined by internal considerations. If it were true that the LXX is on the whole a better text than the Heb.-Sam., and that the latter differs from it mainly in the way of explicable corruption, then we should

certainly conclude that the LXX's text stands nearer to the original than that of Sam. or MT. But since the case is notoriously otherwise—the readings peculiar to the LXX being in most cases inferior to those common to Sam. and MT, and therefore corruptions of a text faithfully preserved by them—the inference is irresistible that the text of the LXX, whatever the age of its archetype, has undergone degeneration, either in Hebrew or Greek MSS., since it parted from the common tradition.

With that explanation, I think that the theory under discussion has a claim to consideration as a feasible solution of an intricate problem ; though there are two reasons that make one hesitate to accept it. One is that tradition and historical probability are against it. What history tells us is that the Samaritan Pentateuch is older than the Greek translation. If we dismiss the statements of the pseudo-Aristeas as unworthy of credence, there is still a probability that the Jews of Alexandria would not have been content with a version derived from any less authentic source than the official Palestinian text of the time. Still, if it should be found that the relations of the three recensions can only be explained by allowing a higher antiquity to the Hebrew archetype of the LXX, the historical presumption may be overridden by literary evidence, and the view we are considering may have to be adopted. But in the second place we may hesitate to decide that this is the only or the best solution of the textual problem. We should have to inquire whether the resources of the theory of Gesenius

(see p. 114 above) are not adequate to the situation with which we have to deal. In other words, whether the history of the text is not more truly exhibited by *this* diagram than by the one already given: Sam. and LXX being both derived from an *unofficial* Hebrew recension (*a–b*), which had ramified into an indefinite variety of texts, of which the Sam. and the LXX are the only

surviving representatives. The subject is too large to be investigated here; but it is not clear to me that the textual facts might not be explained by some such hypothesis as this; in which case the *common element* of the Sam. and LXX would take us back to a point considerably nearer the original text than the actual adoption of the Pentateuch by the Samaritans. For the present, however, I will assume provisionally that the truth lies in the direction of the hypothesis we have been discussing. Let us see how this will

affect the evidence drawn from the Samaritan Pentateuch for the original use of the divine names.

(1) It is obvious that the positive testimony of the combined Sam. and MT remains exactly as before. It remains certain that the names for God as they stand in MT (neglecting the nine variants of the Sam.) were found in authoritative Hebrew MSS. of the fourth century B.C. The only new element is the *assumed* existence of still earlier MSS. in which a different distribution of the names occurred. (2) We have just seen that this assumption only suggests a bare *possibility* that the LXX has preserved the divine names in a more original form than the MT. Undeniably variations have occurred in one line of transmission or the other, but whether these be due to accident or caprice or deliberate tendency, there is no presumption whatever that they are on the side of the MT. (3) From the point of separation of Sam. and MT, Jewish and Samaritan scribes were exceedingly careful in transcribing the names of God, while we have no evidence that the same accuracy obtained in the Hebrew ancestry of the LXX, as it certainly did not obtain in Greek MSS. Are we to suppose that this fidelity of Jewish scribes to the text dates only from the time when the Pentateuch was taken over by the Samaritans? Is it not a reasonable assumption that great care had been exercised in this respect from the beginning by the central authorities in Jerusalem? (4) It is a legitimate supposition—conceded in fact by Wiener, although we can have no absolute certainty on the point—

that the Samaritan Pentateuch was derived from the canonical text of Palestinian Judaism. On Wiener's theory we are bound to suppose that the MS. used by the LXX translators was not an officially guaranteed MS., but a private codex, distant (it may be) by many removes from the central stream of tradition, though it happened to conserve some readings superior to the standard text. Now it is only the official guardians of a canonized text who are in a position to exercise an effective control over its transmission. We know that the Jewish authorities did not succeed perfectly—although they succeeded much better than the copyists of the LXX—in maintaining a pure text; but it does not follow that their efforts were wholly in vain, or that in what they regarded as an important feature of the text—the divine names — they have not preserved the original readings. (5) The LXX, even in cases where we can be sure that its readings rest on a Hebrew original, would only give us the text of a Hebrew MS. extant at the time of translation. We have no proof that it was then an ancient MS., or that in readings like the divine names, whose intrinsic value cannot be judged of, it did not deviate widely from its assumed ancestor. (6) Finally we have to remember that the LXX in its best established text agrees with the MT in about *five-sixths* of the divine names of Genesis, that the remaining sixth may well be due either to the translators not attaching importance to literal exactness in their work, or to inadvertent changes natural to Greek copyists, and that there is not one which might not have been altered in

Greek more readily than in Hebrew:—when we consider all this we shall not be disposed to rate very highly the pretensions of the LXX, whatever be the age of its archetype, to have preserved a more reliable recension of the divine names than the MT or the Samaritan.

It is right to say before passing from the subject that neither this theory nor that of Dr. Kennett has been before the minds of critics in investigating the structure of the Pentateuch: their task has been performed under the influence of a conception of the text springing from the common opinion as to the dates of the LXX and the Sam. So far as I know these theories are both recent arrivals. Now that they have arrived it will be necessary to examine them on their merits, and in the light of all the circumstances of the case. Such an examination is perhaps at the present moment premature. We shall do well to await the publication of a critical edition of the Sam. before committing ourselves to a final judgment upon them. The general acceptance of either view would certainly destroy or weaken the confidence hitherto felt in the Sam. as the earliest witness to the Hebrew text, and would to that extent affect the textual argument for the documentary theory. Even in that event, however, it seems to me that the considerations advanced above are sufficient to dispel the notion that by following the track of the LXX we shall get nearer the original distribution of the divine names than if we trust the Massoretic text.

## V

# OTHER ANCIENT VERSIONS

BESIDES the LXX and the Samaritan Pentateuch, there are several versions of the Old Testament or of the Pentateuch, made directly from the Hebrew, which can be cited as collateral evidence of the condition of the text at different points in its history. With perhaps one partial exception (the so-called Targum of Jonathan), they all belong to an age either contemporary with or later than the fixation of the *textus receptus* which we now possess. Hence, as might be expected, their divergence from our present Hebrew is slight; and *as a rule* it is only where they lend each other mutual support, or agree with LXX or Sam., that any value whatever attaches to their variant readings. That is true of the text in general, but it is conspicuously true of their use of the divine names, in so far as that can be regarded as textual evidence at all.

1. We may first of all dispose of the JEWISH TARGUMS, from which little or nothing can be learned as to the current text of the divine names. These versions are based on the oral translation into the Aramaic vernacular which accompanied the reading of the Law in Jewish Synagogues. We do not know when this oral translation

was first committed to writing; but it is certain that extensive changes continued to be made long after it had assumed the written form. Even a late reading may, however, through oral tradition, attest an early form of Hebrew text. Only two complete Targums of the Pentateuch have survived, one used in the synagogues of Babylonia, and the other of Palestinian origin.

The Babylonian Targum (commonly known as the Targum of Onkelos) is of absolutely no use for our present purpose. For the name of God it employs indiscriminately a symbol of the Tetragrammaton (יי) which completely obliterates the characteristics of the Hebrew text on which the translation is based. Whether this represents the oral usage of the Synagogue we cannot tell; but it certainly does not reflect the contemporary Hebrew text, and therefore it does not concern us here.

The Palestinian Targum (Targum of Pseudo-Jonathan) in its present form is a compilation not older than the eighth century after Christ, and composed of very heterogeneous elements. In one place (Deut. xxxiii. 11) it introduces a reference to the enemies of John Hyrcanus which must date from the beginning of the first century B.C.; in other places it contains allusions to Constantinople (Num. xxiv. 19) and even to the wives of Mahomet (Gen. xxi. 21). Its rendering of the Hebrew is highly paraphrastic, being of the nature of a popular and Midrashic commentary rather than a strict translation. Nevertheless it is frequently possible to discover under its renderings traces of a Hebrew text slightly differing

from the MT. But again it is impossible to make any use of its treatment of the divine names. Its divergences from the MT, which are numerous, are all in one direction; i.e., it usually substitutes J for E but *never* E for J.* In chap. i. 1–ii. 3 Elohim (אלקים) † is consistently retained; in chap. ii., iii., Yahwe (ה׳ for השם, "the Name") is used in iii. 3 for Elohim, and in iii. 5 for Yahwe Elohim; elsewhere in these two chapters the double name is employed, and in iii. 1 takes the place of E in the MT. From iv. 1 onwards the use of J tends to become exclusive: but E is retained in vii. 16*a*, ix. 1, 6, 8, 16, 17; after ix. 17 J is invariably used except in chap. xx. where (apart from an omission in xx. 13) the divine names agree with those of MT. We can clearly form no conclusion as to the distribution of names in the Hebrew basis of the translation. We may note in passing that it is here if anywhere that we might look for traces of a usage governed by the Seder- or Parasha-division in Palestinian synagogues; but no such principle can be discovered. No conclusion is possible except that we have in this Targum an earlier phase of the tendency to a uniform use of J which reaches its culmination in the Babylonian Targum.

2. We have next the important Greek version of AQUILA. This is a mechanically literal translation of the Old Testament, in a form which might have been intelligible, but could hardly

* I use Ginsburger's edition.

† The proper Aramaic name for God אלהא is used for אל and for אלהים where it has generic or appellative significance.

have been readable, to a Greek. There is no doubt that its intention was to supplant the LXX in the hands of Greek-speaking Jews, and to substitute a correct representation of the authorized scriptures. It aims accordingly at reproducing every minute peculiarity of the Hebrew text—for by that time Jewish exegesis had learned to extract profound meanings from the letter and even the accidental minutiae of the sacred text—in conscious defiance of Greek grammar and idiom. Its great importance lies in the fact that it emanated from the circle of Jewish scholars by whom the Old Testament canon was finally determined and the standard text fixed: Aquila is said to have been a pupil of Rabbi Aqiba. Hence it possesses an authority second only to that of the official text itself. Its variations from the modern received text have never been thoroughly explored, although enough is known to assure us that they are very slight. How then do matters stand with regard to the divine names? Unfortunately the version of Aquila is known to us only from a few fragments, and in citations and marginal notes taken mostly from the third column of Origen's Hexapla. Of the passages of Genesis where the name of God occurs there are only about thirty-two* where the reading of Aquila has been preserved. In all of them it agrees with the MT, with the single exception of xxx. 24, where, as we have seen

* i. 1, 4, 5, 6, 8, 10, 26, 27, 28*a*, 28*b*, 29, 31; ii. 7, 8; iii. 1*a*, 1*b*; iv. 6, 25; v. 22, 24*a*; vi. 6, 9; viii. 21*a*; xviii. 30; xxiv. 31; xxx. 8, 24; xxxii. 10, 28; xxxiii. 5; xli. 16; l. 19.

already (p. 54 f.: cf. p. 186), Aquila has the support of the Peshitta and LXX, but not of the Sam. nor of any Hebrew MS. We need not here discuss the intrinsic merits of the reading; the important fact is that it is the only divine name in Genesis where the authority of Aquila can be quoted against the MT. It is, of course, only one instance out of thirty-two; but the evidence so far as it goes entitles us to say that the discrepancies between the MT and Aquila must have been too rare to disturb appreciably the data employed in working out the documentary hypothesis.

3. Our next witness is the Syriac version, the Peshitta. Of its origin nothing is certainly known, but it is credibly believed to date from the second century after Christ. The two leading facts as to its character on which scholars are agreed are: first that it is a translation made directly from the Hebrew, but secondly that it has been partly revised in accordance with the LXX. The most difficult question is whether its Hebrew basis was dependent on the archetype of our Massoretic text, or distinct from it. In a recent monograph * this point has been investigated for the book of Genesis; and the writer comes to the conclusion that it is possible to distinguish between variations due to the LXX revision and those native to the Hebrew original, and that the latter prove the Hebrew basis of the Peshitta to have been slightly nearer that of the LXX than the MT is.† To put

* Hänel, *Die aussermasoret. Uebereinstimmungen zwischen der Sept. und der Pesch. in der Gen.*, 1911. See also Barnes in *Journ. of Theol. Studies*, II, p. 186 ff.

† *Op. cit.*, p. 68.

the value of the Peshitta at its highest, we will assume that this view is correct, so that the Hebrew original of that version is a text independent of (though closely akin to) the official Jewish text of the second century. We will also allow that the divine names are an element of the text little likely to have been assimilated to the LXX by revision; and we will not raise the question of the fidelity with which the Syriac text has itself been transmitted.

The actual divergences of the Peshitta from the MT, according to Dahse's tables, are as follows.*

| | |
|---|---|
| ? iii. 11 | + J |
| ? iii. 13 | J for JE (*H.P.* arabs 4) |
| iii. 24 | + JE (fi$^{a}$ Chr.) |
| ? iv. 10 | +J |
| vii. 1 | E for J (Sam. ? K601, 686 cw Arm.-codd.) |
| ? xiii. 10*a* | E for J (LXX) |
| xiii. 10*b* | E for J (LXX) |
| xiv. 22 | Om. J (LXX) |
| xv. 6 | E for J (LXX, Vulg.) |
| xxii. 11 | E for J (K248, 601) |
| xxii. 15 | E for J |
| xxix. 32 | E for J (Georg.) |
| xxx. 24 | E for J (LXX, Aq., Sym.) |
| ? xxx. 27 | E for J (LXX) |
| xxxi. 16*b* | J for E |

* In four of these passages the reading is doubtful. In iv. 10, xiii. 10*a* the Ambrosian Codex agrees with MT; in iii. 13 MT is supported by all the leading editions (Ambrosian, Lee, Urumiah, Mosul) except the London Polyglot; and in xxx. 27 by all except the London Polyglot and Lee. Dahse further queries iii. 11: for what reason I cannot discover. It would appear therefore that there are only ten undoubted variants of Pesh. from MT.

How then does Dahse explain the phenomena of the Peshitta? He regards it as representing a transition stage between LXX and MT in a progressive elimination of Elohim in favour of Yahwe. The oldest translation (the LXX) contains the greatest proportion of Es, the Peshitta has fewer, and the MT fewest of all (p. 51 f.). It appears to me that this view of a *gradual* substitution of J for E is totally irreconcilable with his Pericope-hypothesis. According to that theory the preponderance of E in the LXX was brought about once for all by an editorial operation, in which many a J was deliberately changed to E on principles determined by the Sedarim-division of the Law. Similarly the substitution of J for E in the MT was due to another instantaneous redaction, influenced by the later Parasha-division. And now we are told that there was an intermediate stage, marked by the Pesh., at which the Sedarim-division was abandoned and the Parasha-system not yet introduced! We are, of course, not sorry to find the inconsequent Pericope-theory so feebly rooted in the mind of its author; but unfortunately this new and contradictory suggestion of a progressive elimination of E from the text is equally at fault. In the first place it is setting the facts in a wrong perspective to speak of the Pesh. as having fewer (*schon weniger*) Es than the LXX; the truth being that it has only a very few more than the MT (so Dahse on p. 26). It is surely a thoroughly arbitrary and perverse proceeding to treat the Pesh. as dependent on the text to which it has least resemblance (the LXX), and as independent of the MT, with which it was

nearly contemporary, and with which it so closely agrees. Moreover, in one-third of the cases (three out of nine) where E stands in the Pesh. for a J of MT, E is not read in the LXX; and in two of the six places where E *is* supported by LXX the Syriac text is doubtful: so that there is no clear indication of tendency in the direction imagined by Dahse. It is quite certain, therefore, that as regards its use of the divine names the Pesh. is not a stepping-stone from LXX to MT (or for that matter from MT to LXX). We observe, further, that Dahse, as usual, ignores the existence of the Samaritan Pentateuch. His view of the Pesh. involves the tacit assumption that the Hebrew basis of that version branched off from the official Jewish text at a point later than the original of the LXX, but earlier than the Sam. He has left us in doubt whether he really entertains that extravagant opinion—in spite of the fact that the Sam. exhibits only a single agreement with a distinctive divine name of the Pesh. in Genesis—or whether he has never thought of the matter at all.

The obviously right course is to start from the fact that the basis of the Peshitta was a Hebrew text circulating in the first or second century after Christ, and to compare that text with the contemporary recension preserved in the MT. Looking at the list of variants from this point of view, the first thing that strikes us is the preference for E over J which the Pesh. exhibits in a much less degree than the LXX. There is but one case where an E of MT is replaced by J (Dahse, p. 26 f.), and only two doubtful cases

where J is added to the text. Next, we are struck by the proportion of feebly supported readings. In eight out of the above fifteen variants (iii. 11, 13, 24, iv. 10, xxii. 11, 15, xxix. 32, xxxi. 16) the Pesh. has either no external corroboration or none worth speaking of; it is rarely supported by more than a single independent witness (vii. 1, xv. 6, xxx. 24); it has only one doubtful agreement with a variant of Sam.; and two with Heb. MSS. If these be characteristics of a Hebrew text of the first or second century, we must of course admit that the official recension had not then obtained the exclusive ascendancy which it secured at a later time; but, on the other hand, they furnish a remarkable proof of the solidarity of the Hebrew text of that age. They certainly do not suggest that the text represented by the Pesh. had had a separate history dating from the time when the Sam. or the LXX branched off from the Jewish stem, or that the agreements with the LXX are other than accidental. The only reasonable view is that if the Hebrew basis of the Pesh. was not the MT itself, it was an unauthorized offshoot of the official Palestinian text of comparatively recent origin. And after all we cannot altogether ignore the possibility of errors in the transmission of the Syriac text.

4. The last version that requires notice is the Latin VULGATE, produced by Jerome in the closing decade of the fourth century and the first of the fifth. By that time the Massoretic recension had been established for 250 years—long enough for many mistakes to have crept into MSS. derived from a single archetype. Hence a divergence of

the Vulgate, unless it commends itself by its intrinsic superiority, or is corroborated by textual evidence, can never take us behind the sources of the MT.* As regards the divine names, Dahse cites only three variants: viz., vi. 5 E for J (= K 80); vii. 9 J for E (= Sam., K155); xv. 6 E for J (=LXX, Pesh.): to these must be added vi. 3 (E for J).

I am well aware that the affinities of these versions with one another and with the MT, and their evidential value for textual criticism, are questions that cannot be decided on the narrow basis of the divine names. Their relations must ultimately be determined by characteristic readings whose inherent value can be estimated, and whose lineage can be traced, with some assurance that we are not dealing merely with accidental coincidences. That investigation has yet to be undertaken for the versions now in question. Nevertheless the divine names do constitute to some extent a special problem; and I think we are warranted in maintaining: first, that the Jewish Targums count for nothing in this inquiry; and second, that Aquila and the Vulgate strongly confirm the Massoretic text. The Peshitta alone presents a doubtful aspect. But even if we concede the utmost importance that can possibly be claimed for its 10 or 11 or even 15 discrepancies, they are too few to discredit the general soundness of the Massoretic text, or to invalidate critical conclusions founded on the assumption of its trustworthiness.

* See NOTE IX, p. 281.

## VI

# THE LIMITS OF TEXTUAL UNCERTAINTY

IT is now time to gather up the threads of this protracted and sometimes, I am afraid, intricate discussion, and to consider with unprejudiced minds how it fares with the documentary theory of the Pentateuch, after the most elaborate assault that has yet been made under the banner of "textual" criticism. I have pointed out, in the introductory paragraphs (pp. 6–11), that Dahse, like others of his school, greatly exaggerates the importance of the divine names for the analysis of the Pentateuch,* but no one will deny that they have a certain importance, or that if, as regards the names for God, the text with which critics have operated could be shown to be either demonstrably wrong or hopelessly uncertain, the evidence for the documentary hypothesis would at some points (at least in the analysis of J and E) be sensibly weakened. In succeeding sections I have examined at great length the new and positive suggestions that Dahse has brought to bear on the problem, and hope I have convinced my readers that he has failed to substantiate any one of them. I have called attention incidentally

* See NOTE X, p. 288.

to certain fallacious assumptions, errors in reasoning, and misstatements of fact, which detract from his argument, and seem to me to reveal a bias in favour of anything that makes for the strengthening of his own speculations. If it were a mere matter of repelling a particular attack, it would hardly be necessary to add anything to what has already been said. But the controversy raises a wider issue than that. It will doubtless have left on the minds of some of my readers an impression that after all said and done the situation does not remain exactly as it was before. It will be felt that even if every specific argument has been fairly met and successfully refuted—and I do not assume that this will be universally granted—yet the general precariousness of the textual data is so much greater than has hitherto been realized that confidence in the results of critical analysis must be seriously shaken. I apprehend, in short, that the real effect of Dahse's work will be rather the diffusion of a vague uncertainty* as regards the Hebrew text in general, than the solution of any problem by the light of new constructive principles. I will therefore endeavour, in this chapter, to sum up the arguments for and against the trustworthiness of the Massoretic text, and to show that within the margin of uncertainty which admittedly exists, there is a solid and sufficient working basis for the literary analysis of the Pentateuch, and for such use of the divine names in that analysis as a reasonable criticism requires to make.

* See NOTE XI, p. 290.

I.

In order to clear the ground for such a statement, I will first ask the reader's attention to the following examination of Dahse's view of the *status quaestionis*, which is given on pp. 51 f. of his book. After a critical review of the history of the controversy up to the present time, he sums up the considerations that bear on the rival claims of the MT and the LXX to represent the original names, under the following heads:

A. In favour of the Massoretic text (and against the LXX):

1. The agreement of the Hebrew MSS. with the Samaritan and with one another, and the absence of any tendency towards assimilation; and,
2. In the LXX, errors and carelessness on the part of translators and copyists, alterations due to religious motives, aversion to יהוה, partiality for ὁ θεός.

We shall see in a little that the case for the MT and against the LXX might be put a good deal more strongly than that. For my part, I lay no stress on the "religious considerations" or the deliberate avoidance of Yahwe in the LXX; and the phrase about "absence of tendency to assimilation" conveys no meaning to my mind, and was probably not intended by Dahse to carry much weight. But in what remains—the unimportance of Hebrew variants, the remarkable harmony of

Sam. and MT and the notorious confusion of the LXX text—there are solid grounds for maintaining the superiority of the Massoretic tradition, and grounds which will not be easily neutralized by the motley array of considerations which Dahse marshals on the other side. Let us see what these are.

B. Against the originality of the Massoretic text, and in favour of the higher antiquity of the LXX, we are asked to consider—

1. That there was no hesitation about changing the name of God in Hebrew writings, as is clear from
   (*a*) the Books of Psalms and Chronicles,
   (*b*) the testimony of the Talmud, and
   (*c*) the Targum.

In Hebrew writings! In another connexion Dahse himself protests against citing the analogy of the later books of the Canon as evidence against the fidelity of the Greek translation of the Pentateuch; and says that in regard to the originality of the divine names *in the LXX* each book must be examined separately (p. 25). He cannot run with the hare and hunt with the hounds. If that is a good rule when the accuracy of the LXX is in question, it surely applies *a fortiori* to a question of the soundness of the MT. But let us waive the point, and look at his proof: (*a*) the Psalter contains evidence that certain Psalms were subjected to an Elohistic redaction, which of course means

that no scruple was felt in altering the name of God in the compilation of a part of the Temple liturgy. On this subject it is enough to refer to my note on p. 40, where it is pointed out that this operation was performed on writings not yet invested with canonical authority, and therefore furnishing no presumption that a similar licence would have been permitted in dealing with the divine names in the Law.—The case of the Chronicler is more pertinent, and I admit that it gives us to think seriously, though not exactly in the way that Dahse imagines. There appears to be no doubt that the Chronicler allows himself considerable freedom in the use of the names Yahwe and Elohim. When writing independently, he evinces a preference for Elohim, especially in the phrase "house of God" (21 times); and—what is more to the purpose—in making excerpts from Samuel and Kings, he not infrequently uses Elohim where the MT of these books has Yahwe.* It follows that either the Chronicler or the compilers of the canonical Samuel and Kings have in some cases altered the names found in the original sources. Dahse (p. 44) quotes from Eerdmans a sentence to the effect that the age of the Chronicler, "in which Yahwe and Elohim were used promiscuously," is the age from which "the older writings" have been handed down to us; the inference being

* 1 Chron. xiii. 8, 12, 14, xiv. 10, 11, 14*a*, 15, 16, xvi. 1*a*, 1*b*, xvii. 2, 3, 17, xxi. 8, 17; 2 Chron. i. 7, xviii. 5, xxxiii. 7, xxxiv. 27; and eleven times in "house of God," 2 Chron. iv. 11, 19, v. 1, 14, vii. 5, xv. 18, xxii. 12, xxiii. 3, 9, xxv. 24, xxxiv. 9.

that the text of these older writings was not then handled with the same scrupulous care as in later times. But against this we must observe *first*, that the freedom exercised by a compiler in *making extracts* from written sources is no evidence that a similar licence was used by scribes in *copying* a sacred text. *Secondly*, that in the time of the Chronicler the books of Samuel and Kings had not been admitted to the canon of sacred scripture, and were therefore not protected by an official censorship against irresponsible changes of text. *Thirdly*, the Samaritan Pentateuch proves that the transmission of the divine names of the Law was *not* influenced by the lax usage of the Chronicler; for the Samaritan Pentateuch had been in existence from 330 B.C., while the books of Chronicles cannot be dated earlier than about the year 300, and may have been written even a century later. It is clear, therefore, that the indiscriminate use of the divine names in the third century B.C. has had no effect on the text of the Pentateuch. The real point at which the argument from the somewhat loose practice of the Chronicler touches the problem before us is the misgiving (whatever it may count for) that redactors of the Pentateuch may have been as reckless of the distinction between Yahwe and Elohim as the Chronicler was in his day. That is a consideration to which due weight must be allowed; but its importance must not be exaggerated. We can never, of course, find direct proof that the compilers of the Pentateuch accurately transcribed the names of God as they stood in the original sources; at

the same time the probability of their having done so is not greatly lessened by our knowledge of the inconsistent usage of a much later age. The question is whether any presumption created by the practice of the Chronicler (and the Elohistic redaction of the Psalter) is not negatived by other considerations to be advanced in the later part of this chapter.—(*b*) We come to the alleged proof from the *Talmud*. Dahse refers (p. 21) to the statement in *Sanhedrin* (fol. 103*b*) that Manasseh "cut out the divine names" (האזכרות) — not, however, "from the books of the Old Testament," but (as is obvious from the context) from the Law—and blandly asks, "Does not that point to great alterations that had taken place in the use of the divine names in course of time?" It is difficult to deal seriously with such a wild suggestion. To cut out the divine Name means simply to treat the scriptures as a heretical book (*Sabb.* 116*a*); and how the imaginary accusation of a monstrous sacrilege like that could be alleged as evidence of a well-known Jewish practice Dahse will on reflection find it hard to explain.*—(*c*) The

* The connexion in which the statement occurs in the Talmud is the following: "Ahaz abolished the worship, and sealed up the Law, as it is written [Isa. viii. 16] . . . , *Manasseh cut out the divine names*, and pulled down the altar, Amon burned the Law and caused cobwebs to cover the altar; Ahaz committed incest, Manasseh went in to his sister, Amon went in to his mother." There seems to be some derangement of clauses in the ordinary editions; but the intention is clear. Ahaz, Manasseh and Amon represent three grades of wickedness: Ahaz sealed up the law, Manasseh desecrated it, Amon burned it. Unless Dahse has some proof of the contrary in reserve, we shall hold

*Targum* is at any rate not Hebrew literature; and here again Dahse is answered out of his own mouth. On p. 50 he remarks that it is "direct falsch" to assume "on the ground of the Targums that a Yahwe is original in the Hebrew text, for, as has been repeatedly observed, in the Targum Yahwe is also the equivalent of Elohim." That is the common sense of the matter; see the last chapter, p. 137 f. But if "the citation of the Aramaic paraphrase by Ball proves nothing whatever" (p. 50), how can it prove anything when cited by Dahse?—Let us proceed to the next head:

2. That the variants of the LXX are supported by
   (*a*) Hebrew MSS.,
   (*b*) by Aquila, Symmachus, ὁ Ἑβραῖος, ὁ Σύρος, Pesh., Vulg.,
   (*c*) by the witness of the prophetic writings,
   (*d*) by ancient proper names,
   (*e*) by internal considerations.

(*a*) I have dealt fully with the variants of Hebrew MSS. in the fourth chapter (pp. 89–104 above) and shown that the value of such corroborations is *nil*. (*b*) It is true that *a few* LXX variants in Genesis are supported by *one or other* (very rarely by *two*) of the authorities here cited: viz., one (xxx. 24) by Aquila * and Symmachus; one

that the second assertion is just as destitute of traditional warrant as the other two.

* Symmachus does not count as an independent authority for the text of the divine names. There is reason to believe

(iv. 1) by ὁ Ἑβραῖος * and ὁ Σύρος; six (xiii. 10*a*, xiii. 10*b*, xiv. 22, xv. 6, xxx. 24, xxx. 27) by Pesh.; † and two or one (iv. 1 ?, xv. 6) by Vulg.‡ That is all. On the other hand, the MT. is supported *against* the LXX: six times by Aquila; four times by Symmachus (i. 28*b*, ii. 7, iv. 1, viii. 21), and by Pesh. and Vulg.‡ in all cases where there is a divergence, except the few mentioned above.—(*c*), (*d*) and (*e*) have been examined on pp. 105 ff., not exhaustively, indeed, but quite adequately by way of example; and I have simply to repeat that there is no single reading of the LXX which

that his translation was a revision of that of Aquila. Twenty-one of his readings containing a name of God are recorded by Field in Genesis; and I have found no case where he reads a divine name differently from his predecessor. Of the authorities cited as ὁ Ἑβραῖος and ὁ Σύρος nothing whatever is certainly known. If Field is right in conjecturing that they were individual translators from the Hebrew, one a Jew and the other of Syrian nationality, the question still remains whether they did their work independently of the LXX. For our present purpose it is of very little consequence; for iv. 1, 2 are the only cases where a divine name of the former is preserved, and ὁ Σύρος is also represented by only two readings, viz., iv. 1 (see above) and xii. 8, where it agrees with both MT and LXX.

* In iv. 26 the reading of ὁ Ἑβρ. is dubious.

† See p. 141.

‡ That is, in the ordinary text, as cited in Dahse's tables. The MSS. of the Vulgate exhibit as great variations in the use of the divine names as those of the LXX; and it is rather surprising that neither Dahse nor Wiener seems to have explored this interesting realm of confusion. Still more surprising is Dahse's ignoring of the Sam. See NOTE IX, p. 281.

can be successfully vindicated on any of these grounds.

> 3. Yahwe is in certain places a later insertion, so that the contention that the LXX, out of a shrinking from Yahwe, translated it by κύριος falls to the ground. This insertion of Yahwe is proved,
>
> (*a*) by Origen,
>
> (*b*) by the MT itself.

I confess that the reasoning of this paragraph eludes my comprehension. Dahse has argued (pp. 13, 40 f.) that in certain passages (xiv. 22, xv. 2, xxiv. 40, 48, xxviii. 13, xxxii. 9) the Yahwe is a later addition to the MT. I do not think he has made this probable except in xiv. 22, and *perhaps* xxxii. 9;* but even supposing he were right in all the cases, how does that show that the translators of the LXX did not shrink from rendering Yahwe by κύριος? I have never held that they had any such shrinking; but all that the alleged facts can possibly show is that certain late editors of the *Hebrew text* were not influenced by the feeling in question. Nor do I see what proof of the alleged additions can be drawn either from the MT or from Origen. It appears to me that any semblance of proof that is given rests on MSS. or recensions of the LXX which

* In xxxii. 9 κύριε is omitted only by **dp**. Dahse's statement (p. 13) would lead one to suppose that these MSS. are supported by **egj** and **fir**; but **egj** and **i$^{a}$r** differ from the ordinary LXX (= MT) only by *adding* to κύριε, ὁ θεός (μου).

*differ from* Origen, and on Dahse's inveterate tendency to assume an independent Hebrew original for any reading found in LXX MSS. which differs from the MT.

4. A comparison of the MT and Pesh. with the LXX shows that the original Elohim-passages have steadily diminished, inasmuch as
   (*a*) the oldest translation contains most of them,
   (*b*) the Peshitta fewer, and
   (*c*) the MT fewest of all.

That statement has been examined in the last section (p. 142 f.), and shown to be entirely arbitrary and misleading.

5. The distribution of divine names in the LXX of Genesis i.–ix. 26 proves the existence of an Elohistic redaction, which, however, is older than the Yahwistic redaction in the MT, for
6. In the MT regard is had to the Parasha-division.
7. From all this it follows that in respect of the divine names there must have been various editions of Genesis, on which more light will be thrown in the course of the following investigations.

These three propositions stand or fall with Dahse's pericope-hypothesis, and his theory of

recensions, which have been dealt with in the second and third sections of this volume. I have there shown that the former fails to account for the textual facts; and that, while the existence of the alleged recensions in the Greek text is an open question, not the slightest evidence has been produced that there were corresponding recensions in the Hebrew. It thus appears that Dahse's summing up of the case, when closely scrutinized, leaves the balance of evidence decidedly in favour of the MT. The thinly beaten-out argument for the superiority of the LXX resolves itself for the most part into a mixture of unfounded speculations with exaggerated estimate of facts. The only items of evidence to which any real weight can be assigned are those numbered 1 (*a*) and 2 (*b*) above; and even if one were disposed to allow some indeterminate value to the other considerations, their cumulative effect would be small. Arguments require to be weighed as well as counted; and on any impartial estimate the two solid grounds of confidence in the MT far outweigh the trivial and sometimes fantastic observations that are thrown into the opposite scale.

## II

Textual criticism, as practised by Dahse, is a combination of three processes: *first*, the determination of the oldest text by documentary evidence; *second*, the attempt to establish the original reading by internal considerations; and *third*, the formation of hypotheses to explain the variations which the text has undergone in the course of its transmission. The second method,

we have seen, is inapplicable to the case of the divine names; and the third, although a perfectly legitimate process in itself, has so far proved illusory even in the skilful hands of Dahse. It remains to consider whether by the first method alone we can compass a solution of our problem, or whether it leaves us in such uncertainty as to render abortive *ab initio* any attempt to recover the original readings of the sources of the Pentateuch.

(1) It is a sound critical maxim that the correctness or originality of a reading is not to be questioned when it presents no inherent difficulty, and when all documentary evidence is united in its support. It has already been shown that there is no case where a reading of the divine Name can be certainly accepted or rejected on *internal* grounds; it must now be added that a considerable number of readings cannot rightly be challenged on *external* or documentary grounds. In all such cases the true text must be regarded as established. But this principle is set at nought, if not by Dahse himself, at least by his allies Wiener and Schlögl. The former, it appears, is of opinion that only in the rarest instances can it be ascertained whether the original text of Genesis read Yahwe or Elohim (Dahse, p. 32 f.)—just enough instances, I suppose, to prove that the MT is not to be trusted! Schlögl carries his scepticism so far that he feels himself at liberty to change every J in Genesis into E, even in cases—some thirty in number—where E is not read by a single text (see above, p. 31, *note*). If the range of uncertainty were really as great as these two

writers imagine, there might be some excuse for this drastic procedure; but their uncritical handling of the material leads them to overestimate the extent of the divergence. In any case, it is surely the abnegation of textual criticism to set aside the unanimous testimony of MSS. and versions to a particular reading, merely because the documents are at variance in a certain number of other places.

(2) The area of uncertainty with which we have to reckon is pretty nearly measured by the divergence between the MT and the LXX. No doubt the Sam. and the younger versions deviate in a few cases (about fifteen in all)* from MT and LXX, where these two are in agreement. But these are mere family differences, which need not be taken into account until we have settled the much bigger question of the relation of the LXX to the Hebrew text as a whole. Now the "textual" critics seem to me to go astray by not observing that the LXX counts only as a single witness (whether for or against the MT), that its original text must be recovered before it can be cited as evidence against the MT, and that when recovered it cancels all the variants in its MSS. and daughter-versions.† That *we* are uncertain in many cases what the original LXX was, does not alter the fact that it must have been either one thing or another, and not two things at once, or three. It is the first business of the textual critic to ascertain what the best reading of the LXX is: if that reading turns out to be the same

* See TABLE V.

† See below, p. 241 ff.

as the MT, he must obviously forego the liberty of appealing to the weaker reading as evidence of the uncertainty of the MT. If, on the other hand, the true LXX proves to be irrecoverable, he must remember that that result neutralizes the testimony of the LXX, and does not directly affect the credit of the MT.

(3) Here, however, we are confronted by the supreme difficulty of ascertaining in many cases what the reading of the original LXX was. But in this investigation the advocates of textual criticism deliberately adopt principles which can only be characterized as a loading of the dice against the MT. Dahse (p. 31) quotes with strong approval four rules laid down by Wiener for determining the value of LXX variants. We will examine them. (*a*) The first is to the effect that where all LXX authorities agree *in opposition to the MT*, we may be certain that they preserve the original reading of the LXX. I venture to think that the unanimous reading of all LXX authorities must be accepted as the original LXX *whether it differs from MT or not.** Thus we find in Dahse's tables over eighty divine names (about one quarter of the whole number) to which no internal variants of the LXX are recorded; and it is important to observe that there are only five of these cases where the LXX goes

* It is true that there are cases where all LXX MSS. agree in what is manifestly a Greek corruption, and therefore not the reading of the original LXX. But if that has happened in any of the divine names, we should never be able to find it out. The documentary evidence is the final authority in this case.

against the MT (iv. 4, iv. 26, xiii. 10*a*, 10*b*, xxx. 27). By Wiener's rule the original LXX would only be established for these five names. I maintain, on the contrary, that in all the eighty odd places the original LXX is as well established as in the nature of the case it can ever be.—(*b*) We are told that where some LXX authorities support the Hebrew, while the variant is supported by "strong" LXX authority, the variant will be the original reading of the LXX. On this I remark again that the variant will be the original LXX reading, if the authorities in favour of it are sufficiently "strong" to be decisive. But the maxim involves, besides, a plausible abuse of a critical canon which is valid only when it is clear that the Greek variant rests on a Hebrew basis different from the MT. If of two LXX readings one comes under that description, and the other is a close rendering of the existing Hebrew, so that the former cannot have arisen through Greek corruption from the latter, then it is obvious that the first retains the true text of the LXX, and the second is a later accommodation to the received Hebrew text. (It is, of course, a further question which of the two *Hebrew* readings is the original.) But the rule cannot be applied at all in cases (such as the divine names) where we can never be sure that the variant presupposes a divergent Hebrew, and where the liability to inner-Greek corruption is at least as probable an explanation as the tendency to assimilate to the later Hebrew.—(*c*) The third rule, that where Origen is known to have altered the text in conformity with the Hebrew, the unaltered text will

be the original reading, even if all or most other LXX authorities support the Hebrew, is a truism, and therefore correct in substance; although it is not easy to see how we can know of an Origenic alteration except through "other" LXX authorities.*—(*d*) The last rule, that where Lucian alone goes against the MT his text represents an original Hebrew variant, though not necessarily the original text of the LXX, is simply a precarious personal opinion.† So much for the four critical principles to which Dahse has pinned his faith, and which in his opinion vindicate Wiener's claim to be taken seriously as an authority in textual criticism. As for "other canons" which were to emerge in the course of Wiener's inquiry,

* What Wiener is capable of meaning by such a statement may perhaps be gathered from a flagrant example of his carelessness or ignorance on p. 26 of his *Essays*. That page is occupied with a list of "those readings in Genesis ii., iii. for which Hexaplar information is available." In the last line he boldly states, on the authority of Field's Hexapla, that in iii. 23 LORD was added to the text (by Origen), although he cannot cite a single MS. to that effect. A glance at Field's additional note to chap. iii. reveals the source of Wiener's error. There, sure enough, Field marks the κύριος of iii. 23 as a Hexaplar addition; and gives his MS. authority for so doing. But unfortunately Field follows a different numbering of the verses from Wiener; and what is there correctly said of iii. 23 is simply what Wiener has already given in the line before, under iii. 22! The merest tyro might have seen that he was on a false scent when he failed to find an important reference of Field confirmed by Brooke and M'Lean. I will add that if Wiener had read the Greek before him he could not have made this colossal blunder, for the word εἶπεν would have showed him that our *v.* 22, not *v.* 23, was referred to.

† See below, p. 244 f.

Dahse does not refer to them; and as I am not now dealing with Wiener, but with Dahse's estimate of Wiener, I will leave them unnoticed.

(4) In the present state of LXX criticism, we have no more satisfactory means of discovering the divine names in the original LXX than the old-fashioned method of consulting the oldest and best MSS. The study of minor recensions may conceivably in the future bring us nearer to a determination of the true LXX than we are at present; but the results of such study are as yet too problematical to help us in dealing with so variable an element of the text as the divine names of the LXX undoubtedly are. And while Dahse's researches in this direction are no doubt important, they would be more promising if he did not allow them to be "side-tracked" by his too facile and improbable assumption of separate Hebrew originals. In the meantime, at all events, if we are to make any practical use of the LXX at all for the text of the divine names, we must be content to work mainly on the basis of existing MSS. (whether of the original Greek or of daughter-versions), carefully eliminating those readings which are marked as Hexaplaric alterations, and for the rest following the guidance of the oldest and most carefully written codices. Now, I fully admit that this rule about the best MSS. imposes a task of the utmost delicacy on the investigator; and I have no pretension to speak on such a question otherwise than with great diffidence. Nevertheless, after the best examination I am competent to make of the MS.

evidence supplied by Dahse, I will express the belief that the readings of the divine names in the Sixtine and Cambridge editions (which seldom differ) * represent in the great majority of instances the consensus of the best MSS. Making allowance for some doubtful cases, I find that there are about sixty passages where the Cambridge edition reads a different name from the MT.† The number of relevant occurrences of one or other of the divine names in the MT of Genesis is about 320 (in the LXX nearly 330); hence the cases in which the standard LXX throws any doubt on the accuracy of the MT number three-sixteenths of the whole. Roughly speaking, we may take it that that fraction expresses the extent of the "margin of uncertainty" with which criticism, if it had only documentary statistics to guide it, would have to reckon in the divine names of Genesis.‡ But, as

* See NOTE XII, p. 290.

† See TABLE I.

‡ I take this opportunity of correcting an underestimate of the ratio of differences to agreements which I published in the *ICC* (p. xxxv), and of qualifying the conclusion which I drew from that calculation. The error arose partly from taking Redpath and Eerdmans' figure (50) for the divergences, and partly from reckoning the total occurrences as 340 instead of about 320. I suggested that the percentage of variants (one in seven) was probably not so great as to affect the result of literary analysis. I now see, not only that the number of differences is somewhat greater than I stated, but that I failed to allow for the extraordinary *concentration* of the differences in the earlier chapters (ii.–x.). There can be no manner of doubt that in these chapters the divergences are so numerous that the analysis would be altogether impossible if we held only those

we shall see immediately, there are other considerations which enter into the problem, and throw the weight of authority decisively on the side of the MT.

(5) On general grounds, the MT has substantial claims to be preferred to a variant of the LXX in all doubtful cases. (*a*) The MT is the result of successive transcriptions in one and the same language; the LXX is a translation from one language into another. It is not denied that a version *may* represent a purer text than a recension in the original language; but in the absence of proof that this is the case, the presumption is all in favour of the original, because it is not subject to the uncertainty which inevitably attends the mental process of translation; especially when, as is abundantly clear in the case of the LXX, word-for-word translation was not aimed at.—(*b*) The MT is the lineal descendant of the official Palestinian recension of the OT; the LXX represents at best an Alexandrian

readings to be established which are common to LXX and MT, and if the analysis depended on the names alone. On the other hand, the argument is, of course, all the stronger as regards the later chapters, where the proportion of divergences sinks at times to about one in eleven. There is one curious and inexplicable circumstance which may be mentioned here for what it is worth. The double name κύριος ὁ θεός occurs almost exclusively in chaps. ii.–x. Now these readings must have arisen in most cases through conflation, and therefore, are not original; and if we discount them (i.e., if we suppose the MT to have preserved the original names) we find that the proportionate occurrences of LXX variants to MT are pretty evenly distributed over the whole book, although still considerably higher in the early chapters than in the later.

recension whose text was certainly not transmitted with the same scrupulous fidelity as that of Palestine.* For (*c*) as regards the divine names, the Samaritan Pentateuch shows that the Palestinian text has undergone practically no change from a time prior to (or at all events not much later than) the separation of the Palestinian and Egyptian recensions. The LXX text, on the contrary, has been in a state of perpetual flux as far back as its history can be traced. It makes no difference whether this be due to accident or (as Dahse has tried to show) to deliberate revision: on either view the fact remains that the names of God have been handled with a freedom which was not allowed to Jewish scribes.—(*d*) While the LXX contains particular readings which are shown by internal evidence to be superior to the Hebrew, yet an examination of its general text proves that on the whole it is inferior to the Massoretic Hebrew. I do not think that this will be disputed by any competent Old Testament scholar. The MT is often emended from the LXX, but practically never except for some superiority, real or supposed, attaching to the reading presupposed by LXX in particular cases.—(*e*) The liability to error is far greater in Greek than in Hebrew. In the original text we have the distinction, not easily overlooked, between a proper name יהוה and a generic name אלהים. In Greek we have only the difference of two appellatives κύριος and θεός (often contracted in MSS. to κς and θς), a difference without much significance to a Greek-speaking writer, and there-

* See NOTE XIII, p. 291.

fore apt to be effaced through the natural predilection for θεός.

(6) From these observations we may now deduce the principles to be chiefly applied in using the LXX as an instrument of textual criticism. They are succinctly stated by Driver as follows (see his *Genesis*, *Addenda* II, p. xlv f.).* It must be shown

* Comp., to the same effect but more fully, Swete, *Introduction to the O.T. in Greek*, p. 444 f. : "In dealing with such differences between the Greek version and the traditional Hebrew text the student will not start with the assumption that the version has preserved the true reading. It may have been preserved by the official Hebrew or its archetype, and lost in the MSS. which were followed by the translators : or it may have been lost by both. Nor will he assume that the Greek, when it differs from the Hebrew, represents in all cases another Hebrew text ; for the difference may be due to the failure of the translators to understand their Hebrew, or to interpret it aright. His first business is to decide whether the Greek variant involves a different Hebrew text, or is simply another expression for the text which lies before him in the printed Hebrew Bible. If the former of these alternatives is accepted, he has still to consider whether the text represented by the LXX is preferable to that of the Hebrew Bible and probably original. There is a presumption in favour of readings in which LXX and MT agree, but, as we have said, not an absolute certainty that they are correct, since they may both be affected by a deep-seated corruption which goes back to the age of the Ptolemies. When they differ, LXX will usually deserve to be preferred when it (*a*) fills up a lacuna which can be traced to homoioteleuton in the Hebrew, or (*b*) removes an apparent interpolation, or (*c*) appears to represent a *bona fide* variant in the original which makes better sense than the existing text. Its claims in these cases are strengthened if it has the support of other early and probably independent witnesses such as the Samaritan Pentateuch and the Targum, or of Hebrew variants which survive in existing MSS. of the Massoretic text, or in the Q'ri."

(1) "that the (LXX) variant is not due to a paraphrase or loose rendering on the part of the translator, but really depends upon a various reading in the Hebrew MS. used by him; and (2) that this variant reading in the Hebrew has substantial claims to be preferred to the Massoretic text as being the original reading of the Hebrew," by "its yielding a better sense and its being preferable for philological or grammatical reasons." Now in the case of the divine names neither of these conditions can be completely fulfilled. If, indeed, the LXX is unanimous, or nearly so, in reading a different name from the MT, there is some slight presumption that its Hebrew original was different, and so far the first condition is complied with; though even then we cannot be sure that the variation is not due to the translator's indifference to the distinction between the two Hebrew names for God (comp. the case of Job below), or to scribal or editorial changes of older standing than the earliest extant witnesses to the LXX text. But the second condition can never, in the case of a divine name, be realized, for the simple reason that neither sense nor grammar is ever affected by the substitution of one name for another; and I hesitate to admit even an isolated instance here and there in which the LXX reading is decisively to be preferred on such grounds. If, therefore, a textual critic gives the preference to LXX readings, as such, he must be prepared to maintain the *general superiority of its text*; and he is bound in consistency to carry out his principle in his treatment of the text as a whole. But if he essays this he

will speedily land himself in a *reductio ad absurdum* of the critical axiom with which he starts. It is notorious that the LXX contains many readings which presuppose a Hebrew text, not only inferior to the MT, but absolutely inadmissible; i.e., one which no commentator with a regard for the meaning of the passage could possibly accept. Yet if the divine names of the LXX are to be adopted in preference to MT, merely because they are in the LXX, upon what principle can the rejection of these other impossible LXX variants be defended? There cannot be one law for the names of God and another for other variants; and a rule that leads to absurd consequences in the latter case must be wrong from the first.

## III

The initial mistake of the Higher Criticism, according to Dahse, was that its founders took no pains to verify the text of the divine names before using them as a clue to the structure of the Pentateuch. We can now judge from the result of our previous discussions what progress they would have been likely to make if they had laid that caution to heart. "He that observeth the wind shall not sow"; and it is just possible that if the pioneers of the critical movement had known all that Dahse and Wiener could now tell them about the uncertainty of the MT, they would have been deterred from an enterprise which has done more to vitalize the study of the Old Testament than any other contribution that has ever been made. And I think that Dahse, and

especially Wiener, would find life very uninteresting if they had to pursue the barren round of textual studies without the joy of battle against the imposing edifice which has been built on the foundation laid by those rash and misguided men. But, be that as it may, the question for us to consider is not whether textual criticism ought to have strangled the documentary theory at its birth, but whether it is able to destroy it now—in other words, whether the proved uncertainty of the MT makes the use of the divine names valueless as a criterion of diverse authorship.

I remark, in the first place, that apart altogether from the soundness of the MT, the germ of the documentary theory is contained in Dahse's admission that the original (or at least the earliest accessible) text of Genesis had a mixed distribution of the names for God: i.e., there was an alternation of Yahwe and Elohim. This, he says, is the conclusion at which all recent textual critics—Redpath, Eerdmans, Wiener, as well as himself—have independently arrived (p. 41). To be quite fair, I do not know whether Dahse conceives the *original* text to have been mixed in the sense indicated; but he certainly admits it of the oldest text we can now imagine, and he cannot get his own theories under way on any other supposition. And indeed it would be very difficult to account for textual heterogeneity of this kind arising out of a primary simplicity; and we are entitled to infer that the alternation of the names for God was in the documents from the beginning. Now I have already pointed out that this leaves open a distinct *possibility* that the mixed distribution

may be an index to mixed authorship. But we can go further, and say that it is not only possible but *certain* that at least two writers are concerned in the composition of Genesis. That is an inevitable inference—granted the alternation of the original names—from the express statement of Exodus vi. 2, 3. It was a wise precaution on Dahse's part to try to clear his path of this obstacle at the outset; but if there be any section of his book which has failure written over it more legibly than another it is his treatment of the text of these verses. I have dealt with this matter at length in the first chapter, and argued that the writer of Exodus vi. 2 f. could neither have recorded previous revelations of the Deity under the name Yahwe, nor have put the name into the mouth of any of the patriarchs. It is true that Dahse eliminates the name Yahwe from the earlier self-disclosures of God; but he has never suggested that it ought always to be removed where it occurs in human speech before Moses. We see that such passages cannot have come from the same source as Exodus vi. 2 f. Therefore, to put it at the very lowest, there are at least two writers in Genesis: one who could not use the name Yahwe under given circumstances, and another who could and did. But really that is an absurdly narrow restriction of the inference. The natural conclusion is that one writer will have been consistently Elohistic and the other consistently Yahwistic, however many more writers there may be of either class. And with that conclusion we are well on our way to a documentary theory of the Pentateuch. What is

to prevent us from following up the clue? Is it not worth while to make the experiment, whether with the Massoretic text or any other? It is playing King Canute over again for any school of critics to interpose their veto, and say that the attempt should never have been made, and must not be made now.

The next point to be emphasized is that the acceptance of the MT as a basis of operations by no means implies the assumption of its infallibility. No critic of standing has ever imagined that it is immaculate, even in the matter of the divine names. Criticism has gone to work on the presumption that it is substantially accurate, but at the same time with a clear understanding that errors may occur here and there. It is alleged, no doubt, that while critics acknowledge the abstract possibility of mistakes in the text, they take no account of it in practice, but proceed to carve out their documents with a solemn mechanical precision which would only be justified if the authenticity of the divine names were absolutely guaranteed. Now it need not be denied that critics are under a temptation to push their analysis to an extreme in this direction. It is perfectly natural that, having found the distinction of the divine names in the MT a useful clue to the separation of sources, and having no reason to question its correctness in any particular instance, they should follow it out to its last consequences. But there are few, if any, cases where a generally accepted division of documents rests on the divine names alone; and critics would be the first to admit that if any such case existed the uncertainty

of the text would be a consideration of serious importance. The truth is that this objection springs from that exaggeration of the dependence of the documentary theory on the divine names which seems ineradicable from the militant "text critical" mind. It is never realized that the divine names have served their purpose when they have put criticism *on the track* of a distinction of sources which approves itself by many other characteristic differences, and which would have no critical value if such differences did not exist. When this is understood there will be no disposition to cavil at the notion that a true theory may be extracted from an imperfect text.

But it will be asked, Is it credible that the MT has preserved the original names with even the substantial fidelity which is the necessary condition of successful analysis? Granted that it is the best text available, it is still a stupendous assumption that any text can have retained even approximately the names as they stood in the hypothetical primary documents of Genesis! It would certainly be a very remarkable phenomenon. But before we pronounce it incredible, we must reflect that the divine names have in fact been transmitted with only the slightest variation since the fourth century B.C. A century more brings us to the redaction and promulgation of the Pentateuch; and it is only reasonable to suppose that during that century the preservation of the canonical text was as carefully attended to by the Temple authorities in Jerusalem as in the ages that followed. The danger zone is undoubtedly the period from the seventh to the

fifth century, when the oldest Yahwistic and Elohistic documents had been amalgamated, but were not (so far as we know) under ecclesiastical control. That confusion of the divine names might have arisen under these circumstances cannot be denied, but that it was probable is more than we have any right to affirm. We do not know what precautions were taken to safeguard the integrity of the text; and we do know that Jewish scribes were capable of an astonishing degree of accuracy in transcribing the names for God. From the examples given below, it will be seen that it is by no means an extravagant assumption to hold that in the MT we have a substantially correct reproduction of the divine names as they stood in the original documents.*

* An interesting and, if I mistake not, convincing illustration of fidelity of the MT to an original autograph comes to hand as I write, in a paper by Joh. Herrmann on "The Divine Names in the Text of Ezekiel" (*Alttestamentliche Studien, Rudolf Kittel zum* 60 *Geburtstag dargebracht*, pp. 70–87). It is all the more surprising because the MT of Ezekiel as a whole is often very corrupt; and because several of the most recent scholars had come to the conclusion that the divine names in particular had been so tossed about by transcribers that it was impossible to tell what the original names were, while Cornill was of opinion that the LXX is much nearer the original than the MT. The question is as to the distinction between the names אדני יהוה and יהוה (אלהים is used by Ezekiel only in a generic sense). Herrmann shows in a conclusive manner that Ezekiel's usage can be reduced to a few simple and easily intelligible rules, and that with insignificant exceptions these rules are strictly observed in the MT. (1) The double name א׳׳י is used only in three connexions, viz., (*a*) the introductory formula כה אמר א׳׳י; (*b*) in the concluding formula, נאם א׳׳י; and (*c*) in addressing the Almighty by name. Now in the MT א׳׳י occurs 217

The final proof of the essential soundness of the MT, as a guide to documentary analysis, is—the documentary theory itself. Dahse on p. 14 quotes a sentence from De Wette to the effect that the

times, and of these all but 9 are covered by the three conditions just stated. In the formula (*a*) the double name appears 122 times, against 4 instances of כה אמר יי. In (*b*) we have 81 times אי״י and only 4 times נאם יי. Of (*c*) there are only 5 examples, and no exceptions. (2) יי alone occurs 218 times, and regularly in the following cases; (*a*) in the phrase אני יי (87 times, against 5 examples of אני אי״י; (*b*) after a construct state (excluding נְאֻם) (94 times, against 4 exceptional occurrences of אי יי in this connexion); (*c*) יי also occurs 37 times in other connexions. The bare אדני, is used only 4 times, and that in a proverb quoted from the mouth of the people (xviii. 25, 29, xxxiii. 17, 20). It is impossible to resist the inference that Ezekiel's own practice was regulated by the principles here indicated; and that the few exceptions noted represent the amount of error that has crept into the transmission of the Hebrew text. Turning to the LXX, Herrmann finds that in its best text (that of B) יי, except in xx. 38, is invariably rendered by *κύριος*: on the other hand, אי״י is represented 58 times by κς κς, twice by *ἀδωναὶ* κς (but this may be a Hexaplar correction), 143 times by κς, 7 times by κς *ὁ* θς, and 9 times by κς θς. A few LXX variants comply with the rules given above, and are therefore to be regarded as original; but it can no longer be maintained that the LXX is the better text, or that it rests on a Hebrew basis differing from the MT. We need not here enter into the question raised by the peculiar distribution of the various renderings in the LXX; but on this and other points Herrmann's essay deserves careful perusal.

A more familiar case is the indiscriminate use of the divine names in the LXX of Job. It is well known that the scene of that book is laid outside the land of Israel, and that the problem of retribution is supposed to be discussed on a basis of what we may call natural religion. Hence in the Dialogue the name Yahwe is carefully avoided (it occurs

correctness of the MT is vouched for by its careful retention of "the characteristic features of the various authors, and of the separate pieces of

only in xii. 9, in a passage which is obviously interpolated); and archaic names for God are almost exclusively used: viz., *El* (55 times), *Eloah* (41 times), and *Shaddai* (31 times); (*Elohim* only six times). In the prose Introduction and Epilogue, on the other hand, as well as in headings of speeches, Yahwe is freely employed (30 times), along with 12 cases of Elohim, mostly in connexions where the general name of God is alone suitable. It is again impossible to doubt that here the MT has in the main preserved the names used by the author, and that the translators of the LXX have failed to reproduce the original readings. Thus, if we take the B text of Swete, we find from Hatch's Concordance that in the Prologue and Epilogue, while Yahwe is regularly rendered by κύριος (once κς ὁ θς), Elohim is represented 3 times by κς and 4 times by θς: in the Speeches, *Elohim* is rendered once by θεός, 4 times by κύριος, and once by κύριος ὁ θς; *El* 37 times by κς and twice by θς; *Eloah* 19 times by κς and 8 times by θς; and *Shaddai* 9 times by κς, and 16 times by παντοκράτωρ (once κς παντ.). (I omit the cases where *El* is rendered by ἰσχυρός and *Shaddai* by ἱκανός, because these are peculiar to the text of Theodotion, from which Origen supplemented the genuine LXX. It is possible that some of the other renderings given were also Theodotion's; but enough will remain to prove the indiscriminateness of the original LXX.).—Dahse will hardly maintain that here the LXX has a purer text of the divine names than MT, or that the book of Job must have existed in several Hebrew recensions. He says that in regard to the originality of the divine names each book must be considered by itself. I agree. But that does not at all affect the general principle here insisted on, that Hebrew copyists were capable of a degree of exactness in handling the names of God which was not attained by Greek translators and copyists, because they lacked the instinctive sense of difference which was native to the Hebrew mind. See NOTE XIV, p. 294.

which many books are composed"; and comments on it as follows: "Thus the very point in dispute—the originality of the MT—is taken for granted, and the possibility is overlooked that what are called characteristic features of the various authors may be merely peculiarities of the MT." That seems to me a very hasty and inept criticism. Amongst the distinctive characteristics which De Wette had in mind there are many which no version could obscure, and which cannot possibly be regarded as peculiar to the MT. I will show this immediately; and it will be found that De Wette's statement is perfectly unimpeachable. What it means is simply this: that the names of God in the MT have been accepted as a tentative clue to the literary structure of the Pentateuch, that this clue has led to the discovery of many characteristic differences between different strata of the history and legislation, and that these results by their coherence and mutual compatibility furnish convincing proof that the initial assumption was well founded.

Now to illustrate this proposition in detail would be to write a critical commentary on Genesis. At the close of an article already too long, it is not possible to exhibit the full strength of the argument. But without attempting a demonstration we may look briefly at a few selected examples which will at least vindicate the principle of the argument and show that it is not one to be contemptuously dismissed as a mere *petitio principii*. I believe they will also make it clear that no theory based on the synagogue lectionary

can explain the distribution of divine names in Genesis.

(i.) One of the earliest, and now most familiar, results of documentary analysis was the recognition of two separate accounts of the creation in Genesis i., ii.* They are distinguished not only by material differences of representation—as in the order of the creative works—but by stylistic differences of the most penetrating kind, as well as by a few technical characteristics in expression, such as ברא in chap. i. and יצר in chap. ii. No one who has once had the contrast pointed out to him, and is gifted with a feeling for literature, can fail to perceive that in passing from one narrative to the other we enter a new world of thought, or to have conveyed to him an irresistible impression of diversity of authorship. Moreover the transition from the one account to the other is clearly and even abruptly marked. It comes in the middle of *v.* 4 of chap. ii.; and just there in the MT the name of God is changed: instead of Elohim we have Yahwe Elohim. Is this to be set down as an accidental circumstance? No, replies Dahse, but it is due to the fact that just at this point a new Seder commences. Well, let us see. It is certain that the new Seder does not explain the other and deeper differences that are observed—differences of conception, of tone and atmosphere, of language. These must have been in the record before the Seder-division was made; and why not the distinctive use of the divine names as well? What is the use of a pericope-theory which deals only with a single

* See NOTE XV, p. 294.

feature of the text, and leaves so many other characteristics utterly unexplained? Has it never occurred to Dahse that instead of the divine names being determined by the Sedarim, the Sedarim may have been in part determined by the divine names? That is the obvious explanation in this case. Or, to state it more accurately, the lectionary is arranged in large measure in accordance with the sense. The scribes saw that there was a break at ii. 4, and therefore they chose that as the place where a new lesson should begin.*

That this is the true explanation appears when we look at the end of the second Seder at iii. 21. There we have an instance of unintelligent division, for it is clear that the story of Eden is continued to the end of the chapter. But we frequently find throughout Genesis that in the Seder-division a regard to the sense is interfered with by a marked tendency to commence a lesson with a divine utterance; and that is why Seder 3 begins unnaturally at iii. 22. Now in the MT the double name for God is continued, past the Seder-division, to the close of the Paradise-story. It is evident, therefore, that in this case the use of the divine names does not follow the lectionary, but the literary affinities of the composition; and there is thus good reason to suppose that it was established in the text before the latter was divided into separate sections.

(ii) Chap. iv. is a section uniformly Yahwistic, with the exception of Elohim in *v.* 25. I cannot here enter on the discussion of the relation of

* See p. 223 f. below.

this chapter to chap. ii., iii., or the significance for criticism of the double name in the latter. If the reader will refer to any good commentary on Genesis he will learn that there is reason to think that the facts of the MT, down even to the isolated E of iv. 25, furnish a key to the literary composition of the sources. But I cannot adduce that as evidence here.

(iii) At the beginning of chap. v. we come upon reminiscences of the style of chap. i., ("create," "likeness of God");* and again the change to Elohim marks the transition. The genealogy thus introduced extends to the end of the chapter, as does the use of E, except the one J in v. 29, where again it has a critical significance for the finer shades of analysis which cannot be expounded here. In vi. 1–8 we have a section of entirely different character, commencing with a fragment of ancient mythology, in which the name J alone appears (five times). Now in the fourth Seder these verses are included along with chap. v.; and the change from E to J at v. 29 is inexplicable by any plausible modification of the pericope-hypothesis. On the other hand, it is explained by the hypothesis of different documents.

(iv) We come to the story of the Flood (Parasha II = vi. 9–ix. 17), which is a crucial passage as between the documentary and pericope theories. On the one hand we have an alternation of J and E in which the Sedarim-division is completely ignored.† On the other hand, it is certain that

* And, it should be added, a great many other characteristics of the Priestly Code.

† In Seder 6 (viii. 1–14) no doubt we have a uniform

in this section there is a dovetailing of two flood-narratives, which can be separated with remarkable precision. They are distinguished by a great variety of characteristic differences: they assign different physical causes for the flood (one a forty days' rain, the other a breaking up of the fountains of the great deep); in one the flood begins to subside after 40 days, in the other the waters continue to rise for 150 days; one distinguishes clean and unclean animals and brings the former by sevens, the latter in pairs, into the ark, while the other admits only one pair of each species without ceremonial distinction; one speaks of the sexes as "man and his wife," the other more prosaically as "male and female"; and so on. Again, portions of the story are duplicated (e.g., vi. 17–22 || vii. 1–5; vii. 7 || vii. 13; and many more). Now the analysis which has resulted in the discovery of all these distinctions started from the alternation of the names J and E; and from first to last it has never found occasion to discard that clue as misleading; that is to say, there is no case where the use of J or E conflicts with the other indications of authorship which have emerged during the investigation. I do not say that there may not be cases where the analysis is determined solely by the divine names, and where therefore a mistake in the transmission of the name vitiating the analysis is a possible

use of E; but there are only two instances! In Seder 5 (vi. 9–vii. 24) the names are: EEEEEJJEEJ; and in S. 7 (viii. 15–ix. 17): EJJJEEEEEE—a distribution which from *that* point of view is promiscuous, and irreducible to any principle or rule.

contingency. But if there be any such case it is only where the other indications are indecisive,* and admit of two equally (or almost equally) feasible solutions of the literary problem. That in every crucial instance the various lines of evidence converge, and lead to the isolation of two independent and almost continuous narratives, is a strong proof that J and E are distinctive of two primary documents, and that the MT has preserved the peculiarities of these documents with singular fidelity.

(v) In chap. x.–xvi. the only distinctive divine name that occurs in the MT is J ; † and consequently these chapters afford no illustration of our immediate theme. But in chap. xvii. we come suddenly on an E-section interposed between two J-sections. This chapter forms a Seder by itself —another example of intelligent division of the Law, assisted perhaps by the abrupt change from J to E in the fundamental text. At first sight it seems open to Dahse to claim that his hypothesis gives at least as good an account of the names as the documentary theory. But that claim cannot be allowed. It is not at all clear why the authors of the lectionary, or those who manipulated the divine names in accordance with its divisions, should have kept up the monotonous use of J through nearly six Sedarim, or a Parasha and a half, and then all at once have resolved to introduce a little variety.

* Thus in vii. 9 the indications are so conflicting that it is difficult to say whether the אלהים of MT, etc., or the יהוה of Sam., Vulg., etc., is the true reading.

† אל in chap. xiv., xvi. 13 does not count.

Besides, the diversity of authorship is a fact. Chap. xvii. abounds in expressions which critics have learned to recognize as peculiar to the Priestly Code, and which on any view do not occur even singly, not to say in such profusion as here, in any of the passages marked by J. Unless the pericope-hypothesis is prepared to face the explanation of this phenomenon (which of course it cannot do), it is idle to pretend that it is the last word on the problem of the divine names. It is true that the first name in the section is J, and that its presence is not explained by the critical hypothesis. But may it not fairly be set down as the exception which proves the rule?

(vi) The alternate use of J and E is resumed at chap. xx. That chapter ( = Seder 17) is itself in MT Elohistic (except *v.* 18); chap. xxi. (S. 18) and xxii., xxiii. (S. 19) are mainly so (except xxi. 1*a*, *b*, 33; xxii. 11, 14*a*, *b*, 15, 16). Can the facts here be explained on the theory of Elohistic authorship, varied by occasional insertions from Yahwistic sources? The answer is that criticism, still following the guidance of the divine names of the MT, finds no reason to distrust it, but, on the contrary, discovers that it is frequently confirmed by independent considerations. The detailed proof of this assertion, however, cannot be given here; and I content myself with citing in conclusion one or two instances of *parallel narration*; i.e., the occurrence of two (or three) different versions of what is obviously a single incident or legend, which have been kept separate without any attempt to weld them (as in the story of the Flood) into a connected composition. We will take four examples.

(*a*) The account of Abraham's denial of his true relation to Sarah in order to save his life in a foreign country. This is first told in xii. 10–20 in a thoroughly Yahwistic connexion (cf. J in xii. 17); then with characteristic variations in chap. xx., which is Elohistic except in the explanatory addition of *v.* 18. A third version of the incident, with Isaac and Rebekah in place of Abraham and Sarah, is given in a Yahwistic dress in xxvi. 7–11.

(*b*) The story of the flight or expulsion of Hagar and birth of Ishmael in chap. xvi. (Yahwistic) and xxi. 8–21 (Elohistic).

(*c*) The patriarchal treaty with Abimelech of Gerar and naming of Beersheba, xxi. 22–31 (*mainly* Elohistic) and xxvi. 26–33 (Yahwistic).

(*d*) The naming of Joseph in ch. xxx. 23, 24.

Now we do not for a moment dispute the contention that a writer, especially a collector of old traditions, might record two or more versions of the same incident without perceiving their original identity. But what is not very credible is that a writer should invariably distinguish his parallels by using J in the one and E in the other. Yet this is done in the four cases before us. In the first (*a*) there are two J narratives, a fact which points to the existence of two strata within the Yahwistic document, but does not in the least obscure the significance of the parallelism between the documents J and E where it occurs. The second (*b*) is entirely free from complications: there are two narratives, one purely Yahwistic and the other purely Elohistic. In (*c*) we have two versions of a single episode; though there

are grounds for thinking that in xxi. 22–31 two narratives are interwoven: one (22–24, 27, 31) marked by Elohim, and the other (25, 26, 28–30) containing no divine names, but *probably* Yahwistic (see *ICC*, p. 325). We do not really need to trouble ourselves here with this refinement of analysis: the fact remains that we have at least two parallel narratives, one Elohistic and the other (xxvi. 26 ff.) Yahwistic. The important point is that the book of Genesis contains examples of dual narration, and that the names J and E closely follow the line of cleavage marked out by the parallelism. The mere existence of duplicates is itself a strong indication of composite structure; and when this is reinforced by a distinctive use of the divine names it surely counts as evidence that J and E are characteristic of two main documents, and can safely be employed as a criterion of authorship. The fourth case (*d*) brings us back to the reading of MT in xxx. 24 (see p. 54 f.). In the naming of Jacob's children we encounter at least three times a double etymology: Issachar (xxx. 16, 18), Zebulun (20) and Joseph (23, 24). In the last of these, different names of God (23 E, 24 J) are found in MT. Dahse says (p. 44) that he is old-fashioned enough to think that one and the same writer might record two etymological word-plays in the cases of Isaac (xxi. 6), Reuben (xxix. 32) and Joseph. —I remark in passing that the number of such word-plays is much greater than he states. We have three assonances of the name Ishmael with the verb שמע (xvi. 11 J, xvii. 20 P, xxi. 17 E), three of Isaac with צחק (xvii. 17 P, xviii. 12 f. J, xxi. 6 E),

besides those of Issachar, Zebulun and Joseph. (There *may* be two in the case of Reuben, but in the present state of the text the second is undecipherable).—We find, then, a recurrent duplication (in some cases triplication) of etymological fancies which in two clear cases follows a division of sources unmistakably marked by other indications. It is surely the most reasonable assumption that wherever a double etymology occurs, we have to do not with the exuberance of "one and the same writer" but with two of the writers of whose work there is such clear evidence throughout Genesis. And when we have such abundant proof that one of these writers used Yahwe and another Elohim as the name of God, we shall certainly not be in haste to conclude that the distinction of names in xxx. 23, 24, is due to an error of the MT, even though there happens to be a certain amount of textual authority against it. It is on this ground that I hold that the Yahwe of MT in xxx. 24 (with Sam. and all Heb. MSS.) has the best claim to represent the original text; and that the Elohim of LXX, Pesh., Aq. and Sym. is a mistaken assimilation to the name in the preceding verse.

We cannot pursue this subject further. If we were to extend our survey to all the cases where two narratives have been worked into one, we should find much additional ground for confidence in the substantial soundness of the MT in its transmission of the divine names. But I venture to think that even the few illustrations that I have been able to give are sufficient to show that the Massoretic recension has led criticism on the

right track in its effort to disentangle the sources of Genesis. It is difficult to imagine that any one who has fairly and carefully considered them will endorse the confidently reiterated assertion of Dahse, that the names of God in Genesis have nothing whatever to do with the variety of documents of which the book is composed.

## VII

# THE PROBLEM OF THE PRIESTLY CODE

THAT the Pentateuch presents to criticism a very complex literary problem is a fact not less obvious from Dahse's constructions than it is from the standpoint of the documentary hypothesis. At the close of an article in the *Studierstube* for July, 1913,* he has given us a sketch of the different processes through which he supposes the material of the Pentateuch to have passed before it arrived at its present condition. Instead of operating with the "obscure entities" J, E and P, he says that in future we shall have to recognize the following strata in its composition: first a *Grundstock* of the Pentateuch; next one or two Prophetic redactors; then a Liturgical redactor (Ezra); and lastly a Theological redactor: not to mention a staff of glossators quite as numerous as has ever been called into requisition by advocates of the documentary analysis. This, for a theory still in its infancy, is a pretty liberal admission that in this case at all events simplicity is not the seal of truth. No doubt, the documentary theory in some of its recent developments

* "Wie erklärt sich der gegenwärtige Zustand der Genesis?" p. 20.

—with their $J^1$, $J^2$, $J^3$, $E^1$, $E^2$, $P^g$, $P^h$, $P^s$, etc.—presents a somewhat formidable aspect of complexity, but its main lines are clear enough. When we consider how much work remains to be done before the new theory can adjust itself to all the details of the analysis, we may reasonably fear that in its ultimate form it will develop a complexity quite as great as the most elaborate form of the documentary hypothesis.*

In the third section of his *Textkritische Materialien*, I (p. 144–174), Dahse attempts to show that he has discovered a "new key" to the structure of the Pentateuch. The discovery of a new key implies the recognition of an old problem; and the problem is one which in varying forms has confronted criticism from the time of Astruc downwards. It is nothing less than the problem

* That Dahse's position is subject to fluctuation is seen when we compare his utterances of February and December, 1913. *Then* (*Die neueste Wendung*, etc.) he did not deny "dass die Mosesbücher aus *Quellen* geschöpft haben," but only demanded that later accretions should be removed from the text before we proceed to determine the exact limits of the documentary sources. *Now* (Reply, p. 506 f.) he says that in all his writings he has never admitted the existence of "parallel documents as sources of the Pentateuch" (he may say what he likes, but *Quellen are* documents as sources and whether they are parallel or not is nothing to the point) and only believes in "different strata" in it. He now concentrates (with Sellin) on the conception of "a holy book which later has undergone revision in the time of the Prophets, in the time of the introduction of lessons into public service, and finally in the time of the Sopherim, to adapt it to the changed circumstances." I admit that *that* is different from a theory of sources; but it is also different from Dahse's position in February, 1913.

of the Priestly Code. Now, properly speaking, this is a question which lies outside the task I set to myself when I began to write on "the Divine Names in Genesis." It has nothing directly to do with the divine names.* But there are two considerations which bring this subject within the compass of the preceding discussions. In the first place, I have myself insisted that the really vital points in the critical position are the relations of the combined JE, of Deuteronomy, and of the Priestly Code; and that these relations are established on grounds independent of the use of the divine names (p. 10). Hence an attack by Dahse on one of these central positions demands attention for its own sake. In the second place, the Pericope-hypothesis is in Dahse's system as essential an element of the "new key" as it is of his theory of the divine names; and although I have proved its complete failure as an explanation of the distribution of the names, still it is open to my opponent to say that I have not done justice to his position, inasmuch as I have left out of account important evidence for the antiquity of the Pericope-system and its influence on the redaction of the Pentateuch. For these reasons, then, I now proceed to subject this theory of his to a patient examination, emphasizing once more, as a point agreed on, that a problem exists, and that the question now between us is which of two critical theories affords the best solution of the phenomena which each seeks to explain.

* The same is true of the second section on "Jakob und Israel," with which I have no occasion whatever to deal. It has been sufficiently "riddled" by Sellin and Gressmann.

Before entering on details it may be desirable to give the reader a general view of the differences between the rival hypotheses. It would be correct, from either standpoint, to speak of certain passages as forming in some sense a "literary framework" of the book of Genesis. On the documentary theory this framework is composed of *disjecta membra* of what was originally an independent work called the Priestly Code (P). At the final redaction of the Pentateuch that document was divided up into sections, longer or shorter, and these were interspersed with the other material of the Pentateuch, the operation being performed with such care and skill that the document can still be read as a continuous, though often attenuated, narrative, in which few lacunæ or dislocations can be detected.* Dahse holds, on the other hand, that the "framework" (or, as he prefers to call it, "liturgical *Beiwerk*") consists of a series of annotations which never had a separate existence. Naturally Dahse's editorial *Beiwerk* is not exactly identical with the critics' P. Still the two schemes (as will be seen from the lists below) † so far coincide that the two theories are

* See *ICC.*, *Genesis*, p. lvii ff., where I have endeavoured to show that in chap. i.–xi. there is no reason to suspect any omission; that in xii.–xxv. 11, though the narrative is reduced to little more than a chronological epitome, yet the fragments of P read so consecutively that they can hardly be mere editorial notes. The same might have been done for the Joseph section from xlvi. 6 to the end. In the remainder of the book the lacunae can be accounted for by accidental omission of a very few sentences from a skeleton history similar to that of Abraham.

† The contents of P as given in Driver's *Introduction*—

mutually exclusive. If Dahse's hypothesis be right, there will certainly not be enough material left to form a continuous document P; and conversely, if the critical analysis be correct there can be no question of resolving the framework into a series of disjointed annotations. And there lies *one* main issue of this discussion.

Thus far, however, there is nothing strikingly original in Dahse's theory. It is in principle the view maintained by Klostermann, and known to many English readers from the late Dr. Orr's book on *The Problem of the Old Testament*. The novel and distinctive feature of Dahse's hypothesis

an impartial authority so far as the present controversy is concerned—(p. 159) are as follows: i. 1–ii. 4*a*, v. 1–28, 30–32, vi. 9–22, vii. 6, 11, 13–16*a*, 17*a* (except *40 days*), 18–21, 24, viii. 1, 2*a*, 3*b*–5, 13*a*, 14–19, ix. 1–17, 28, 29, x. 1–7, 20, 22, 23, 31, 32, xi. 10–27, 31, 32, xii. 4*b*, 5, xiii. 6, 11*b*, 12*a*, xvi. 1*a*, 3, 15, 16, xvii., xix. 29, xxi. 1*b*, 2*b*–5, xxiii., xxv. 7–11*a*, 12–17, 19, 20, 26*b*, xxvi. 34, 35, xxvii. 46–xxviii. 9, xxix. 24, 29*, xxxi. 18*b*, xxxiii. 18*a*, xxxiv. (*passim*), xxxv. 9–13, 15, 22*b*–29, xxxvi. (in the main), xxxvii. 1, 2*a*, xli. 46, xlvi. 6–27, xlvii. 5, 6*a* (LXX), 7–11, 27*b*, 28, xlviii. 3–6, 7?, xlix. 1*a*, 28*b*–33, l. 12, 13.

Dahse assigns to his annotator the following verses or parts (those which coincide with "P" are underlined): ii. 4*a*; iii. 22; v. 1, 2; vi. 9–12; viii. 1, 15; ix. 18, 19; xii. 4, 5*b*; xiii. 11*b*, 12, xv. 19–21, xvi. 3, 16, xvii. 1*a*, 8, 9*a*, 24–27, xix. 29, xx. 18, xxi. 1*b*, 5, 34, xxii. 20–23, xxiii. 1*a*, xxv. 7, 10, 11, 17, 19, 20, 26*b*, xxvi. 34, 35, xxvii. 46, xxviii. 1–9, xxx. 22*a*, xxxi. 3, 18*, xxxv. 6*a**, 9–15, 22*b*–26*, 28, xxxvi. 1–5, 6–8*, xxxvii. 1, 2*a**, xli. 46*a*, xlvi. 6*b*–27, xlvii. 7–12*, 28, xlviii. 1, 2*a*, 3–6 (7?), xlix. 1, 28*, 29 f., (31?), l. 13.

The asterisk (*) denotes that the passage belongs only in part to the "framework."

is the idea that the annotations forming the *Beiwerk* occur for the most part at the beginning or end of a Seder. And that is to my mind by far the most interesting point which Dahse has brought into the arena of investigation. Not, be it observed, that it is by any means a crucial test of the soundness of the documentary theory. Even if it were the case that the P-sections always occur at the divisions between Sedarim, it would still be perfectly conceivable that the editor who took a separate document to pieces made a point of inserting the fragments at the beginning or end of a pericope. There is no reason in the world why he should not—provided the divisions then existed! And it is just in its bearing on this question—the antiquity of the lectionary—that Dahse's suggestion has its importance. If his view is borne out by the facts, it follows that the division of the Pentateuch into reading-lessons is at least as ancient as the last redaction of the Law-book. We should have to conclude either that the latest editor himself arranged the lectionary, or (what is less probable) that he found it in the pre-existing text of the Pentateuch, and made his additions by preference at the introduction or conclusion of a lesson. How important such a result would be, not only for Dahse's various theories but for our whole conception of the history of the Pentateuch, I need not stay to point out.

These, then, are the salient features of the theory we have to examine. It will be convenient to divide the inquiry into two parts. We shall look first at two fundamental passages which sug-

gested the solution to Dahse, and in which we may expect to find it most clearly illustrated. We shall then follow him step by step through Genesis, in order to see whether the view suggested by these test-passages does justice to the structure of the book as a whole. It will be a very tedious investigation; but the three leading questions to be kept in mind throughout are these: (1) Whether the indications on which Dahse relies for the identification of his *Beiwerk* are such as justify themselves on objective critical grounds. In so far as his results agree with the ordinary delimitation of P, we may safely assume that the analysis proceeds on sound principles; for we cannot suppose that two sets of critics occupying such opposite standpoints would agree in their conclusions unless their observations were directed to unambiguous phenomena of the text. But when the two schemes part company we must consider very carefully how far Dahse's analysis rests on real literary criteria, and how far it is dictated by the exigencies of the particular hypothesis which he is concerned to uphold. (2) Whether, taken all together, the isolated passages represent a coherent and independent view of the history as distinct from the earlier tradition. (3) Lastly, whether there is sufficient evidence that the operations of the redactor were regulated by a regard to the divisions of the Synagogue lectionary.

## I

It would seem that the new solution first occurred to Dahse while he was occupied with the

text of Genesis xxxv. 9–15. These verses stand at the beginning of the 32nd Seder; and, with the exception of *v.* 14, are unanimously ascribed by critics to the author of P. Now Dahse observed that xxxv. 15 (naming of Bethel) is a repetition of xxxv. 7, and concluded that it must have been written by a different author. On further inspection he found that everything in *vv.* 9–15 is a duplicate of something already narrated: xxxv. 10 (change of Jacob's name) is parallel to xxxii. 29; xxxv. 12 (promise of the land) to xxviii. 13; xxxv. 14 (setting up the pillar) to xxviii. 18; while xxxv. 11, 13 imitate the phraseology of xvii. 6, 22. The whole passage is thus a recapitulation of xxxii. 25 ff. *plus* xxviii. 13 ff. *plus* xxxv. 7 (*not* xxviii. 19, which Dahse most arbitrarily deletes as a gloss). It is therefore the work of an "Epitomator" or "Kompilator"—we shall call him "K"—who brings nothing new, but emphasizes or elucidates certain points in the older narrative.*

* Dahse's exposition, here and throughout, is encumbered by a mass of textual detail, which seems to me mostly irrelevant to the main points at issue. I do not deny that in some cases the LXX may yield a better text than the Hebrew: where this is the case, and where the difference is material to the argument, I shall take note of the facts; but where the only effect would be to obscure the outlines of Dahse's own theory, I shall ignore it. I do not think that by so doing I shall in any degree weaken my opponent's case. I will say, however, that for the most part his criticism of the MT means nothing more than a prejudice in favour of certain recensions of the LXX—a prejudice which, I need hardly add, I do not share.—In the case before us Dahse's motive is to bring about a closer verbal agreement between the parallels than the Hebrew shows. But accom-

From xxxv. 9–15 Dahse passes to xxviii. 1–9, which stands at the *close* of Seder 25, and is again unanimously assigned to P by the documentary critics. Here we encounter a similar series of parallels, pointing to the same conclusions as before. xxviii. 2 (the sending away of Jacob) repeats xxvii. 43 (?); xxviii. 3 (promise of a numerous progeny) *anticipates* xxviii. 14; xxviii. 4 (promise of the land) *anticipates* xxviii. 13. To xxviii. 6–9 *no parallels are adduced*; instead we are treated to a reconstructed text* which, whether better or worse than MT, throws no light on our problem. It is pointed out further that, on account of xxviii. 5*a*, xxvii. 46 must go along with xxviii. 1–9; and that the mention of Paddan Aram in xxviii. 5 suggests identity of authorship with xxxv. 9—conclusions at which critics had long arrived. In xxvii. 46–xxviii. 9 we have also a new point, which is to be regarded as characteristic of "K": viz., antagonism to mixed marriages with the women of Canaan. Lastly, attention is called to the "significant" fact that both sections

modation of one passage to another is usually regarded as a mark of a degenerate text.

* *v.* 6, *ἰδὼν* (so **fip** $s^{txt}$**v** $c_2$, Arm., Boh., Eth.) *δὲ* Ἡσαὺ *ὅτι εὐλόγησεν* Ἰσαὰκ *τὸν* Ιακὼβ *καὶ ἀπῴχετο*[4] *ἐκεῖθεν* (so **egj**)[5] *καὶ ἐνετείλατο αὐτῷ λέγων οὐ λήμψῃ θυγατέρα τῶν Χαναναίων σεαυτῷ γυναῖκα* (so **n** Bohᵂ. Sah. arabs 3)[6] 8, *καὶ γνοὺς* (so $j^{mg}$ $s^{mg}$; videns Phil.-lat.; *ἰδὼν δὲ* 31) Ἡσαὺ *ὅτι πονηραι εἰσιν αἱ θυγατέρες τῶν Χαναναίων ἐναντίον* Ἰσαὰκ *τοῦ πατρὸς αὐτοῦ*[7] *ἐπορεύθη*[8] 9, *πρὸσ* . . . with *Glosses*: [4]*εἰς τὴν Μεσοποταμίαν Συρίας λαβεῖν εαυτῷ γυναῖκα* (∧ **egj**); [5]*ἐν τῷ εὐλογεῖν αὐτόν* (∧ Eth. $c_2$.), [6]*v.* 7 ∧ **p**; [7]*καὶ* ∧ **p**; Boh. Eth$^{op}$. OL. Phil.-lat.; [8]Ἡσαὺ (∧ E **p** Eth. OL).

stand at the division of Sedarim, one before the break and the other after it.—The theory is practically complete, and the lines of subsequent investigation laid down. We are to go through the book of Genesis to see how often we can find at the beginning *or* end of a Seder passages of similar character to xxxv. 9–15 and xxviii. 1–9.

But let us pause here and consider how far all this really brings us towards an understanding of the structure of Genesis. Looking first at the critical method adopted by Dahse, we see that his principles are precisely those of the higher criticism: only his application of them is vacillating and inconsistent. Thus, he argues that xxxv. 7 and xxxv. 15 cannot be from the same author, because they both record the same fact. It is an excellent maxim, much relied on in documentary analysis; but if it is impartially applied it will carry Dahse much further than he wishes to go. Thus we might say that the same writer would not *twice* record the creation of man (i. 27, ii. 7), or twice the expulsion from Eden (iii. 22, 24), or Noah's entry into the ark (vii. 7, 13), or Laban's going out to the well (xxiv. 29*b*, 30*b*), or Rebekah's departure (xxiv. 61*a*, 61*b*), or twice that Isaac blessed Jacob (xxvii. 23, 27), or that Laban twice asked Jacob to fix his terms (xxx. 28, 31*a*); and so on. Then again Dahse argues for *identity* of authorship from similarities in expression and in thought; but he uses precisely the same argument to prove *diversity* of authorship where the theory demands it. The recurrence of the name Paddan Aram unites xxviii. 1–9 with xxxv. 9–15 in respect of authorship; the warning against mixed mar-

riages in xxviii. 1–9 brings into the same unity xxvii. 46, and is held to be characteristic of "K." But on the other hand the phraseological affinities between xxxv. 11 and xvii. 6, and between xxxv. 13 and xvii. 22 are used to prove that "K" is *not* the writer of xvii. 6, 22, but is reiterating what he found there in the old Pentateuchal text. The higher critics, of course, carry out the principle to its consequences, and hold that P is the author of chap. xvii. It will not be denied that here as regards *method* their procedure is comprehensive and self-consistent, while Dahse's is arbitrary and inconsequent.

But let us come to the substance of the theory. We have seen that in xxviii. 1–9 and xxxv. 9–15 Dahse's "K" agrees with P except as regards xxxv. 14;* and hence the question of the right to treat these sections as *sui generis* does not arise. It would seem, therefore, that the comparison between Dahse's theory and the prevailing critical view turns on two points: first, whether these two passages are of the nature of rubrics, recapitulating or emphasizing features of the main narrative, or are excerpts from an independent document; and second, whether they have anything to do with the Synagogue reading-lessons.

* A difference of opinion as to this verse is the natural result of the two different methods of analysis. Critics refuse to assign *v.* 14 to P, not because it breaks the context, but because it implies an approval of the *Mazzebah* or sacred pillar and the rites connected with it: this is characteristic of E and strongly opposed to the whole tenor of P. It is accordingly regarded as a misplaced fragment of E, whose original position in that document was either after *v.* 8 (so Cornill and others) or in the middle of *v.* 7 (Procksch).

These questions may be considered together. Now (*a*) it is a fact that one of these sections stands at the end of Seder 25 and the other at the beginning of Seder 32. But is it not already a suspicious circumstance that one *does* occur at the beginning and the other at the end? An editor with an eye to the Seder-division would surely have observed a consistent practice, and inserted his observations regularly either at the beginning or at the end of the various Sedarim, but not indifferently, now in the one place and now in the other. I will not press this objection at the present stage, because it might conceivably be overcome if in a sufficient number of cases throughout Genesis a Seder should be found to open or close with a P or "K" section. (*b*) It is true again that each of these passages contains repetitions of what is elsewhere recorded; but it is impossible to show that the repetition has any reference to the Synagogue lessons. We could understand an editor writing a summary of the contents of each Seder at the beginning, or even at the end; we could understand his commencing a new Seder with a résumé of the preceding. But on Dahse's theory "K" *opens* Seder 32 with reminiscences of S. 31 (xxxv. 7), of S. 30 (xxxii. 29), of S. 26 (xxviii. 13, 18), and of S. 14 (xvii. 6, 22); and he *closes* Seder 25 with anticipations of S. 26 (xxviii. 13, 14). Translate this into practical terms and see what it means. It means that on a particular Sabbath the audience in the Synagogue was to be reminded, in the most casual and haphazard manner, of what they had heard two, six, or even eighteen weeks previously; and again that on another Sabbath

their attention was diverted to matters which they were not to hear about till the following Sabbath. Could anything be imagined less helpful to the reading of Scripture than that? Dahse (p. 162) appeals to the chapter-headings in German (or English) Bibles, and asks us to consider what would be the effect if by any chance these headings had come to be printed as part of the text. When he can produce a parallel to the confusion and overlapping which he is compelled to attribute to his "Kompilator" we may begin to see some daylight in his speculations. (c) The theory of recapitulation does not account for the most characteristic phenomena of the sections assigned to "K." They exhibit all the marks of a *duplicate narrative*, resting on an independent view of the history. Dahse admits that the story of Esau's marriages (xxvi. 34 f., xxvii. 46, xxviii. 6 ff.) is quite peculiar to "K"; he might have added that the motive thus supplied for Jacob's journey to Paddan Aram is entirely foreign to the main narrative of Genesis. Can it, then, be fairly said that xxviii. 2 is a repetition of xxvii. 43? In one case (JE) the mother sends away the son that he may escape his brother's vengeance for his treacherous filching of the paternal benediction, and without any hint that he was to find a wife before he comes home in "a few days." In the other (P) the father freely bestows his blessing on Jacob and sends him away for the sole and express purpose of contracting a marriage among his mother's relatives. Is it not evident that the author of this second account is *rewriting the history* from a new point of view? (*d*) Critics

will agree with Dahse that the author of xxvii. 46–xxviii. 9 is animated by a strong antipathy to intermarriage of Israelites with Canaanites. But why should a mere "Epitomator" choose this particular point in the story to emphasize his opposition? There was nothing in the original text to suggest it. Why is he silent at chap. xxiv.—the account of the choice of a bride for Isaac—the one passage in the pre-Exilic Genesis where the objection to such marriages finds expression? Surely that is the opportunity which he could not have missed if he had had any regard to the text before him, or to the case of the Synagogue worshippers! (*e*) In xxxv. 9–15 the order of the main narrative is *reversed* by the transposition of two incidents, the revelation at Peniel (xxxv. 9 f. ‖ xxxii. 25) being placed before the revelation at Bethel (xxxv. 11–15 ‖ xxviii. 13 ff.)—a scene from Seder 30 before one from Seder 26. Such a transposition is intelligible on the part of a redactor piecing together separate documents; but it is hard to justify in the case of an "Epitomator" with nothing to influence him but the pre-existing Pentateuch. Dahse only makes his case worse by appealing to Hos. xii. 4, 5, where we find the same order (Peniel—Bethel) as in "K" or P. For if Hosea and "K" followed a different tradition from the older Genesis, then "K" is to that extent an independent writer, who freely reshapes the history in accordance with what he deems a superior tradition.*

* It may be right to mention that the order in Genesis xxxv. 1–13, 15 has been felt as a difficulty by some recent exponents of the documentary theory. The word עוד ("again")

I have dwelt on these two passages at perhaps needless length, in order to show that they do not afford even a promising "jumping-off place" for Dahse's theory of the P-passages. I submit that he has not made good a presumption in favour of the hypothesis of a liturgical *Beiwerk* composed of sporadic annotations intended as a guide in the reading of the Law. All the phenomena he has pointed out, so far as they are relevant to the matter in dispute, are more satisfactorily explained as characteristics of a separate work than as idiosyncrasies of an editor, who read lessons into the ancient history in no way sug-

in the MT of xxxv. 9 presupposes an earlier appearance of God to Jacob, and none such is recorded *in the document P.* Further, although P occasionally departs from the traditional order of JE, yet as a rule he follows it as his chief authority. Gunkel (with whom Procksch agrees) has accordingly conjectured that *vv.* 11–13, 15 are a misplaced fragment of P's narrative, whose original position was after xxxv. 6*a*, and this again immediately after xxviii. 9, on Jacob's outward journey to Paddan Aram; while *vv.* 9, 10 occupy their present place. The solution is very plausible, and if the "again" be genuine, perhaps necessary. Dahse rejects the עוד on the ground that it is wanting in a few LXX authorities; and it is no doubt *possible* (we cannot allow more) that it was inserted in the Hebrew text at a late stage with reference to the previous revelations to Jacob mentioned in the completed Pentateuch. If "again" be not original another explanation of the order in P would be feasible on the documentary theory. E distinguishes two visits to Bethel, and connects the naming of the place with the second, which took place after the return from Syria. The author of P might have been led by this circumstance to transfer the Bethel theophany of chap. xxviii. to E's second visit, and run the two incidents into one. So, to be sure, might "K"; but on that view he is not a mere "Epitomator."

gested by the immediate context. When we consider that xxvii. 46–xxviii. 9 and xxxv. 9–13 form two solid blocks of continuous narrative, united by phraseological and other affinities, representing a peculiar point of view, and having no exclusive relation to the adjacent text, I think we shall find little reason to fear that the documentary theory has been seriously discredited by Dahse's halting criticism.

## II

In proceeding to the second and more laborious part of our inquiry, we may allow that Dahse has still before him some prospect of saving a little for his theory, even after his failure to establish a basis for it in the two selected passages we have just examined. For although there is sufficient evidence of the existence of a separate document P, from which passages like xxvii. 46–xxviii. 9, xxxv. 9–13, 15, and many others are excerpted, it does not follow that every fragment which critics have assigned to that document really belongs to it. It is conceivable that brief notices which critics have taken to be connecting links between the longer sections of P are in reality redactional glosses supplied by an editor who had the whole Pentateuch before him. If Dahse can show that notices of this character frequently occur at the divisions of the Sedarim, he will not have overthrown the documentary hypothesis, but he will nevertheless have made a useful contribution to the criticism of the Pentateuch. We will endeavour, therefore, to estimate quite fairly the evidence he adduces.

1. He begins with the portion of Genesis intermediate between xxviii. 1–9 and xxxv. 9–15 : i.e., with Sedarim 26–31. In Seder 26 (xxviii. 10–xxix. 30) and Seder 27 (xxix. 31–xxx. 21) he finds no trace of "K." He only ventures to affirm that the last clause of S. 26 ("and he served with him yet seven other years") is "the addition of a glossator," solely on the ground of its omission in three LXX MSS (E fp). The clause is in no way suspicious ; and few would reject it on such slight authority. But even granting that it were a gloss, and that it illustrates a tendency to insert such glosses at the end of a Seder, the observation is still nothing to the point. We can readily allow that very late glosses were more apt to be written at the end of a Seder than elsewhere : what we desiderate is evidence that "K" followed this practice ; and confessedly such evidence is not forthcoming here.—In Seder 28 (xxx. 22–xxxi. 2) Dahse detects the hand of "K" in the opening sentence xxx. 22*a*. This, he says, is the "heading" of the Seder. That is to say, the statement "God remembered Rachel" is the "heading" of a section which devotes three verses to the birth of Joseph and twenty-one verses to the stratagems by which Jacob circumvented Laban and the consequent alienation of Laban's sons ! It is an interesting example of what a heading may be in Dahse's criticism. But there is a more important question. By what right does Dahse assign xxx. 22*a* to the same hand as xxviii. 1 ff. and xxxv. 9 ff. ? There are absolutely no points of contact between them. Some critics assign the sentence to P, because of the name *Elohim*, and

the general resemblance of the clause to viii. 1 and xix. 29. These grounds seem to me (as to Driver) somewhat indecisive; but whether they are cogent on the assumptions of the documentary theory or not, they can have no existence for Dahse. The first he has cut from under his feet by denying that the divine names have anything to do with authorship, and the second vanishes with the absence of any specific resemblances to xxviii. 1 ff. or xxxv. 9 ff. There is thus no justification on his principles for taking out a clause firmly imbedded in the context and labelling it a liturgical addition. With precisely as much right he might break off the first verse or half-verse of any Seder, call it a "heading," and use it as proof of his theory.—In point of fact he does this in the very next Seder (29 = xxxi. 3–xxxii. 3), where he assigns to the *Beiwerk* a "solitary verse (xxxi. 3) of the so-called J," because it "makes the impression" of being a "heading" or introduction to what follows. The verse reads: "And Yahwe said to Jacob, Return to the land of thy fathers, and to the place of thy nativity, and I will be with thee." I venture to think that the impression spoken of will be made only on a mind dominated by a preconceived theory, and pretty hard pressed for facts to support it. Both xxxi. 3 and xxx. 22*a* must be flatly disallowed as evidence for the annotation theory.—On Seder 30 (xxxii. 4–xxxiii. 17) Dahse simply remarks on the fitness of the division (which we are not concerned to dispute); he can point to no trace of the handiwork of "K."—Seder 31 (xxxiii. 18–xxxv. 8) yields at last a slight apparent vindication of the theory.

In xxxiii. 18 the words "in the land of Canaan when he came from Paddan Aram" which occur, not indeed at the very beginning of the Seder, but near it, are regarded by critics as a fragment of P; and Dahse appropriates them for his *Beiwerk*. That is reasonable enough: "K" shows a partiality for the expressions "land of Canaan" and "Paddan Aram." But unfortunately the phrase as here used presents our "K" in an altogether new light. He is no longer a recapitulator of the older history, nor a writer of headings to Sedarim, but a glossator, pure and simple, of an established text. It follows that the occurrence of the phrases near the beginning of the Seder is purely accidental; it was just there that the opportunity for this gloss presented itself. In fact, the same phenomenon recurs, as Dahse admits, in the middle of Sedarim, at xxxi. 18 and xxxv. 6. It is therefore impossible to admit even this as a confirmation of Dahse's hypothesis.

Besides the passages hitherto mentioned, critics find traces of P in xxix. 24, 28*b*, 29, xxx. 4*a*, 9*b*. What does Dahse make of these? As to the first three, he says there is no *need* to detach them from their setting, and no reason for assigning them to "K." As to what may fitly be attributed to "K," Dahse may be the best judge; but critics have quite as good grounds for assigning them to P as xxxi. 18 or xxxv. 6, and in my opinion much stronger grounds than in the case of xxx. 22*a*. And as for the need for isolating them—"O reason not the need!" Was there any *need* to detach xxx. 22*a* or xxxi. 3? In regard to xxx. 4*a*, 9*b*, Dahse takes

refuge in a dissensus of critical opinion. The point is not worth labouring here; I will only take occasion to remark that Dahse is mistaken in supposing that "Gunkel and Skinner" assign these half-verses to P merely to fill out the meagre contents of that document. The suggestion is absurd.

Looking back, then, over the last two paragraphs, we find that the six Sedarim under consideration have not furnished a particle of substantial evidence in favour of Dahse's hypothesis; or, if we include the 25th and 32nd Sedarim, that the theory is very partially verified only in the two passages with which we started, xxviii. 1–9 and xxxv. 9–15. It is with some surprise that we read Dahse's own summing up of the case. "Thus we see how *almost always* on the boundary of two pericopes, either at the beginning or close of a Seder, the hand of our Kompilator shows itself, and how this hand does not show itself elsewhere in these chapters, with the exception of the short additions xxxi. 18 and xxxv. 6" (p. 148). Is that a fair statement of results? Why, even on his own showing there are only five of the eight Sedarim (25, 28, 29, 31, 32) where the hand of "K" appears either at the beginning or the end; and by what arbitrary devices he has made out even that number of coincidences we have now seen. A little lower down (p. 149) we read: "We have therefore *no passage* of the *so-called* P which has not a relation to the division of Genesis into reading lessons." What of xxxi. 18 and xxxv. 6; to say nothing of xxix. 24, 28*b*, 29, xxx. 4*a*, 9*b*, which Dahse refuses

to allow? It may be easy to rush a hasty reader into acceptance of an intricate critical hypothesis by loose and inaccurate statements like these; but more care is required of one who seeks to win over scholarly opinion.

2. Having now, as he believes, entrenched his position in the centre of the book, Dahse proceeds to extend his conquests over the rest of Genesis; and first of all *backwards* from chap. xxvii. to chap. xii. (S. 24–S. 9). And we, on our part, having gained some experience of his line of research, need not follow his track quite so closely as we have done, but may adopt a more summary treatment of his results. But we shall retain the backward order.

Of these sixteen Sedarim the following are untouched by P or "K": 24 (xxvii. 1–27), 21, 20 (= chap. xxiv.), 15 (xviii.) and 11 (xiv.). In the following there is no divergence between P and "K" sufficiently serious to greatly affect the evidence for the theory: 23 (xxv. 19–xxvi. 35: P = "K" in xxv. 19 f., 26*b*, xxvi. 34 f.), 13 (chap. xvi.: P in 1*a*, 3, 15 f.: "K" 3, 16), 10 (xii. 10–xiii. 18: P in xii. 6; P and "K" in xiii. 11*b*, 12); and 9 (xii. 1–9: P 4*b*, 5; "K" 4 and part of 5). The remaining sections, however, call for closer examination.

Seder 22 (xxv. 1–18) contains on the critical theory two lengthy P passages, 7–11*a*, 12–17. According to Dahse "K" is responsible for *vv.* 7, 10, 11, 17. In his opinion the Seder originally ended at *v.* 11, the genealogy of Ishmael (12–18) being omitted by three Greek MSS. (19, 108, **w**).*

* In reality there are only two, 19 (= **b**) and **w**. The verses are *not* wanting in 108 (= *b*), as Dahse will see from

That it is a late insertion in the text is extremely improbable. It opens with a formula distinctive of "K," as of P: "And these are the generations of"; it is given by the Sam., its excision would leave us with the shortest Seder in Genesis, consisting according to Dahse of eight verses (1–6, 8, 9), to which "K" supplied a *Beiwerk* of three verses; while on a more scientific analysis* P is responsible for no fewer than $4\frac{1}{2}$ verses out of the 11. And how little conscience Dahse makes of his textual criticism at this point may be seen from p. 165, where xxv. 17—i.e., one of the spurious verses—is actually ascribed to the "Kompilator" Ezra! We may therefore disregard the assertion that *vv.* 10, 11 mark the end of Seder 22, which thus has nothing of "K" either at

the frequent citation of its readings to these seven verses in the Cambridge LXX. And of the two **w** carries no weight, because its text has evidently passed through the hands of a scribe or editor who had a strong aversion to transcribing long lists of proper names. The tendency is not so marked in Genesis (though compare x. 2–32, xxv. 1–5, xxxvi. 9–43); but it appears very unmistakably in Joshua (see Jos. xii. 2–6, xiii. 8–xiv. 2, xv. 1–xvii. 18, xviii. 12–38, xix. 2–9, 10*b*–16, 18–23, 25–31, 41–47, xxi. 8*b*–40). It is true that in Joshua **w** parts company with **b**, and falls in with another group (*K* **gln**), but that does not strengthen the value of its own evidence in passages full of proper names. (For this information I am indebted to Mr. McLean.)

* *v.* 8 contains two distinctive expressions (גוע and נאסף אל עמיו) never found in the Pent. except in P-contexts (see Driver, *LOT*, p. 131 ff. Nos. 9 and 25); and in *v.* 9 "Machpelah" is just as distinctive of Dahse's "K" as Paddan Aram (see Dahse on xlix. 29 and l. 13). Moreover *vv.* 7–11*a* form a continuous section which there is certainly no "need" to divide between two writers.

beginning or end (although of course *critics* assign 12–17 to P).

We come next to Seder 19 (xxii., xxiii.). Neither at the beginning nor at the end does Dahse claim anything for "K." But at xxiii. i, where a new *Parasha* begins, he recognizes the hand of "K" in xxiii. 1*a*, and (tentatively) in xxii. 20–24. To the second of these suggestions I entirely demur; the first, with its time-determination, would on the *Beiwerk*-theory very naturally fall to "K." Supposing then that "K" has interposed at this point, what follows? Obviously, in the first place, that "K" ignores the *Seder*-division. But next, that he had regard to the *Parasha*-division. Since exactly the same thing happens in the only other case where a Parasha begins in the middle of a Seder (xlvii. 28) I do not see how on Dahse's principles that conclusion can be avoided. Of course it would prove too much for his purpose. It would mean that the Parasha-division is as old as the time of Ezra—much older, therefore, than the LXX—and the whole ingenious construction on which rests the proof of the superior antiquity of the divine names of the LXX—namely, that the LXX is influenced by the older Seder-system and not yet by the Parashas, while the MT is influenced by both—goes by the board. On p. 150 Dahse evades this point, and fixes attention on the gloss שני חיי שרה, which is not in the original LXX, and which he thinks was added as a short summary (!) to mark the new Parasha. But that is entirely immaterial to the present issue.

In Seder 18 (xxi.) the hand of "K" appears in 1*b* (beginning) but also in *v.* 5 (neither beginning nor

end). On the critical theory 1*b*, 2*b*–5 belong to P. The last verse of this Seder, xxi. 34, "And Abraham sojourned in the land of the Philistines many days," which is surely the natural sequel to the treaty between Abraham and the Philistines, is pronounced most arbitrarily by Dahse to be a "concluding postscript," and without even an attempt at justification, is assigned to "K"!— Similarly the last verse of Seder 17 (xx.), "For Yahwe had closed," etc., which is generally regarded as a gloss, is appropriated for "K." But we have already seen (on xxix. 30*b*) that not every gloss, even at the end of a Seder, is the work of "K"; and Dahse can produce no reason whatever for thinking that this one betrays his hand.

In Seder 16 (chap. xix.) the only P-verse is 29; and this is also the only verse claimed for "K." Dahse surmises that it marked the close of the actual lesson, the remainder of the Seder (30–38) being deemed unfit for public reading. If Ezra deemed it unfit for public reading, why did he not remove it from the text, as Dahse supposes him to have done with the sequel to xxxv. 22*a* (Dahse, p. 154 f.)? We decline however to accept a "vielleicht" as proof; and insist on the *fact* that here "K" (P) only interposes in the middle of the Seder.

This brings us to the important Seder 14 (chap. xvii.), the whole of which, as is well known, is on the documentary hypothesis an extract from P. Dahse, of course, cannot acquiesce in this view, although it is based on linguistic, literary and material evidence of the most convincing kind (see *ICC*, p. 289 f.), which he has not even thought it worth while to examine. That this is not a

prejudice due to long familiarity with the teaching of the critical school may be seen from the emphatic verdict of Eerdmans, who says that in style and subject-matter this chapter is akin to xxxv. 9–15 (Dahse's "K"!), and that beyond all doubt the two passages are from the same pen (*Komp. der Gen.*, p. 13). Nay more, it was Dahse's own opinion in 1903, before his critical judgment was altogether perverted by preoccupation with the pericope-hypothesis. In the *ARW* for that year (p. 317) he wrote as follows: "Der ganze Character von Gen. 17 gleicht demjenigen vollständig, der uns sonst in der P genannten Schrift entgegentritt." *Now*, by applying a very few arbitrarily selected criteria, he detaches 1*a* (time-specification), 8 ("land of Canaan," duplication of 7*b* and xii. 7), 24 f. (time-specification) and 26 f. (Abschluss), and assigns them to "K"; leaving all the rest of the chapter to the "Grundstock" of the Pentateuch.* Such fitful and capricious criticism is little likely to stand the test of time.

Lastly, in Seder 12 (xv.), where there is nothing of P, Dahse takes the three closing verses (19–21) for the *Beiwerk*, referring to Neh. ix. 8. But Neh. ix. 8 only shows that the composer of that prayer had Genesis xv. before him with this interpolation already in the text. No reason is given

* But how Joshua v. 2–8 can be held to prove the early date of Genesis xvii. I am at a loss to imagine. And would it not be passing strange if Ezra (who is "K") should write exactly in the style of an author who must have lived so many centuries before his day, even when there was nothing in the context to suggest imitation?

for assigning the verses to "K." This is but one of a series of passages enumerating in similar terms the peoples of Canaan, which are scattered through the Pentateuch and Joshua (Exod. iii. 8, 17, xiii. 5, xxiii. 23, xxxiv. 11; Deut. vii. 1, xx. 17; Josh. iii. 10, ix. 1, xi. 3, xii. 8, xxiv. 11). Does Dahse claim all these for "K"? If he does, his theory will suffer loss; for not one of the others occurs at the end of a Pentateuchal Seder; and the final position here is quite accidental. On the critical theory all these passages occur in JE or Dt. contexts, and never have any relation to P.

3. We turn now to the closing chapters, from xxxv. to the end. Seder 32, commencing at xxxv. 9, extends to xxxvi. 43. It contains a P-passage, xxxv. 22*b*–26 (the list of Jacob's sons), which Dahse regards as a recapitulation of chap. xxix., xxx., and assigns accordingly to "K." Critics have pointed out two discrepancies between JE and P in this matter: first, the order of enumeration, and second, the statement (26*b*) that all, including Benjamin, were born in Paddan Aram. Dahse gets rid of both these by textual criticism. As to the order, he urges that the recension fir has a different arrangement which more nearly (but not exactly!) corresponds with xxix., xxx. The point is hardly worth discussing here.* The more important discrepancy as to

* The extent of the difference is that fir puts xxxv. 24 after 26*a*, thus closing the list with the sons of Rachel. It may be noted, as illustrating the facility with which such transpositions were made, that codices of the Armenian version (which belongs, according to Dahse, to the same recension as fir) place the sons of Bilhah after those of Zilpah.—And Dahse argues very rashly from the agreement

the place of Benjamin's birth is dismissed very summarily with the remark that 26*b* cannot have stood in the same original text as 22 (why not?), and is not found in the Armenian edition (?).*— Dahse places the end of this Seder at xxxvi. 8. He seems to have been the first to call attention to the fact that *vv.* 9–43 are wanting in one Greek MS. (**w**), and that in two others (19, 108) the words of xxxvii. 1 come between xxxvi. 8 and xxxvi. 9. The parent text of these two MSS. must therefore have agreed with **w** in the omission of this section, and the lacuna must have been supplied in them from some other source. That is undoubtedly an important textual fact, although its significance is very easily exaggerated.† In

between the order of **fir** and that followed in the "Testaments of the Twelve Patriarchs." That post-Christian work is so freely interpolated in a Christian interest that it must have been a favourite reading-book in Christian circles. If it had any influence on the recension **fir** (which I do not for a moment believe) that influence is just as likely to have been exerted on a Greek editor as on a Hebrew recension.

* What authority he can have for this unsupported assertion I do not know. No such variant is cited in the apparatus of the Cambridge LXX.

† The omission by **w**, from what we have seen (footnote on p. 208 f.) as to the tendency of that MS., is neither surprising nor of any special moment. 108 (? fourteenth century) is practically a transcript of 19 (= **b** : ? tenth century), so that their united testimony is only that of a single witness. That raises a very interesting text-critical problem, which I have not leisure to work out. Can we suppose that all these omissions occurred in the common original of **b** and **w**, that this original has been faithfully preserved in **w**, while in **b** the missing passages were restored with the (accidental?) exception of xxv. 12–18, and in 108 that last gap was filled up at a still later time? On any view I cannot see that two

any case the question has very little bearing on the present inquiry. Let us suppose that Dahse is right, and that the Seder ends at xxxvi. 8. The whole of the later part of this section (xxxv. 22*b*–xxxvi. 8) is assigned to P; and in the main Dahse agrees, reserving, however, for the original text xxxv. 27, 29, and xxxvi. 6. He thus burdens the original text with a glaring contradiction, when in xxxvi. 6 it puts the migration of Esau after the death of Isaac, whereas in xxxii. 3, xxxiii. 14, 16, it had taken place years before. I will not further discuss the analysis; but the literary result is curious. We have now an extremely short Seder of seven verses (xxxv. 16–22*a*) which "K" has provided with a *Beiwerk* of about nineteen verses on Dahse's view, or twenty-two verses on the documentary hypothesis (xxxv. 9–15 at the beginning and xxxv. 22*b*–26, xxxvi. 1–8 at the end). Why this excessive annotation? To recapitulate portions of the older narrative? But why should such an amount of recapitulation be crowded into one lesson? And why in this remarkable order: first Seder 30, 26, 31, then 27, 28, 32? And whence the *new* material of xxxvi. 1 ff.? It seems to me that this is the *reductio ad absurdum* of the theory of chapter headings.

Seder 33 (chap. xxxvii.) commences with a P-fragment xxxvii. 1, 2*a*. Dahse accepts for "K" *v.* 1 and in 2*a* the words: "This is the genealogy

cursives (virtually one) prove that a peculiarity of this kind belongs to a recension going back to the origins of the Greek text, or that such evidence can neutralize the testimony of *all* other textual witnesses.

of Jacob: when . . . was seventeen years old"—surely a peculiar form for a heading to take! In the MT of 2*a* the enmity to Joseph is confined to the sons of the concubines, whereas in the subsequent narrative it is shared by the sons of Leah. Dahse gets rid of the discrepancy by adding "Leah" to the text on the authority of four cursives (**dnyp**). But this is to beg the question. It is more likely that "Leah" is an addition to the original text than that its omission is due to error; for, accident apart, there was an obvious harmonistic motive for inserting it, but none for deleting it. The presumption is on the side of the MT.

The next seven Sedarim (34–40) call for little remark. In 34, 35, 36, 38, there is no trace of P or "K"; and in 37 (xli. 38–xlii. 17) P ("K") appears only in the middle (xli. 46*b*). Seder 39, which begins inappropriately in the middle of a speech of Jacob (xliii. 14–xliv. 17), contains near the beginning the name *El Shaddai*, which as we have seen (p. 22 above) Dahse regards as a late substitution for *Elohai* ("my God"). I have discussed this view in another connexion, and since the alleged gloss is not assigned to "K" the question does not specially concern us here. Seder 20 (xliv. 18–xlvi. 27) closes with a long P-passage, xlvi. 6–27 (list of Jacob's descendants), which Dahse assigns to "K" (except 6*a*) as an addition to the older history. An *addition*, and therefore not a recapitulation!

In the middle of Seder 41 (xlvi. 28–xlvii. 31) we come to a P-passage (xlvii. 5–11), where critics adopt the LXX text in preference to MT, and

find in it a confirmation of their theory. Dahse, of course, agrees as to the superiority of the LXX, but denies that it favours the usual division of sources. I try to explain his criticism in the note below.* After all, the gain to his theory is *nil*. For, granting that the presentation of Jacob to Pharaoh is not a contrast (though it is uncommonly like a parallel) to the presentation of the five brothers (*v.* 2 ff.), but is a continuation of it, it is at all events not a *recapitulation* of it: it is in fact a fresh narrative, and cannot be the work of a mere "Kompilator." Moreover, the verses stand neither at the beginning nor at the end of the

* I will first of all quote Driver's succinct and lucid note on *vv.* 5, 6 (*Genesis*, p. 370): "Verse 5 is not at all a natural reply to the request in *v.* 4*b*; and there can be no question that the arrangement of these verses in the LXX is preferable to that of the present Hebrew text. After *v.* 4 the LXX continues: '5*a* (J) And Pharaoh said unto Joseph, Let them dwell in the land of Goshen: and if thou knowest any able men among them, then make them rulers over my cattle. 5*b* (P) And Jacob and his sons came into Egypt unto Joseph. And Pharaoh king of Egypt heard of it. And Pharaoh spake unto Joseph, saying, Thy father and thy brethren are come unto thee: 6 Behold the land of Egypt is before thee: in the best of the land make thy father and thy brethren to dwell. 7 And Joseph brought in," etc. (as in the Hebrew). Here the words forming 5*a* in the LXX are a natural and suitable answer to *v.* 4. Now the essential feature of Dahse's reconstruction is that he removes 5*b* and 6 (as given here by Driver) as a late interpolation. That is to say, he practically agrees with Driver as to the old (J) text, but in P he breaks a faultless sequence by dividing it up between a late glossator and "K"! And for this he does not offer a shadow of textual justification. It is all so plain to any one who does not look at the verses through the "spectacles of the newer documentary hypothesis"!

Seder; although Dahse saves himself on this point by putting forward a perfectly baseless conjecture that "vermutlich" a new Seder *once* began at xlvii. 13.—On xlvii. 28, where a new *Parasha* begins, I will not dwell, but simply refer to what I have said above on the similar case of xxiii. 1 (p. 210).

Near the beginning of Seder 42 (chap. xlviii.) critics recognize the hand of P in *vv.* 3–6. Dahse claims for "K" the whole of *vv.* 1–6 except 2*b*, "and Israel strengthened himself and sat upon the bed," which he leaves as the continuation of xlvii. 41, "and Israel bowed himself upon the bed's head," in the original text. That looks plausible so far; but when we resume the original text at *v.* 8 we find Joseph's two sons present, and (without *v.* 1) we have no knowledge how they came there. How then can it be attributed to a Kompilator? Dahse has his answer ready. "K" observed the hiatus, and filled it up in the margin! But one does not go to the chapter headings of English Bibles to supply a hiatus in the Scriptural text. Would it not be much better to leave *vv.* 1 and 2 to the ancient text, and let it tell its own story? These verses contain not a single mark of P or "K."*

* I will here call attention to the extraordinary conclusions which Dahse draws from xlviii. 6 (p. 159 f.; repeated in his Reply, p. 502 f.). That verse, he says, speaks of *sons*, other than Manasseh and Ephraim, who were to be born to Joseph in Egypt; and the Hebrew text knows of no such sons. But, says Dahse, they are mentioned, though not named, in the LXX of xlvi. 27, where we read that the sons of Joseph born to him in Egypt were nine souls. Therefore we have here a clear case where the MT presupposes a text which is only preserved in the LXX! This is a truly astounding proposi-

The last Seder of Genesis (43: chaps. xlix., l.) has fragments of P (on the documentary theory) in xlix. 1*a*, 28*b*–33, l. 12, 13, which read consecutively. Dahse must at all hazards break up the continuity; and he assigns to "K" xlix. 1, 28*a*, *ba*, 29*b*, 30 (save glosses), 31 (doubtfully) and l. 13; to the original text, xlix. 29*a*, 33, l. 12 (LXX.); and rejects as glosses xlix. 28*bβ*, parts of 29, 30, all 32 and (doubtfully) 31. I confess I can furnish the reader with no clue to this labyrinthine analysis. I will only point out that "K" is still responsible for the burial of Jacob in Machpelah (xlix. 29 f., l. 13); and that this is opposed not only to l. 5 but still more to l. 10, where obviously the burial of Jacob takes place somewhere east of the Jordan. It is evident that here also "K" follows a different tradition from the older Genesis, and is therefore no mere annotator of the pre-Exilic narrative.

## III

Let us now sum up the results of our examination.

(1) Dahse has in no instance produced valid

tion. Surely Dahse must see that throughout the summations of xlvi. 6–27 the word *vioí* means "descendants," and includes sons, grandsons, great-grandsons, and even great-great-grandsons. Moreover these "sons" of Joseph *are* named in the Hebrew of *v.* 20; and, except Manasseh and Ephraim themselves, are all grandsons or great-grandsons of Joseph. There are, it is true, only seven of them in all, and xlvi. 27 (LXX) says "nine." But the *ἐννέα* of the common LXX text obviously rests on an error of calculation, for the explanation of which I may refer the reader to the *ICC*, p. 494 f. Has the German scholar no friends to warn him against such pitfalls?

reasons for questioning the accuracy of the prevalent critical delimitation of P. In the main, as we have seen, he follows the line of the much decried documentary theory; and (to change the metaphor) as soon as he discards the use of its borrowed "spectacles" his eyesight fails him. All the positive criteria by which he distinguishes "K" had been noted and consistently applied by critics; and whenever he goes beyond or ignores these his analysis becomes arbitrary and capricious, and is really special pleading for his own hypothesis. The only question, therefore, which he has succeeded in raising is the question of the character and origin of those passages which critics have assigned to P.

(2) Dahse has entirely failed to exhibit an intelligible and self-consistent *modus operandi* on the part of his annotator. We find "K" now engaged in adding a heading or postscript to a Seder; now in epitomizing and now in supplementing the older narrative, and sometimes feebly echoing it; now working in a chronological scheme; once making good a hiatus; and again glossing an established text. It may be said that this is just what might be expected of an annotator; but that does not cover the case. The writing of headings to Sedarim is a process which we must see carried out with some regularity before we can believe in it at all; and we expect recapitulation to be performed with some sense of proportion and some regard to the adjoining text. We have shown that none of these conditions is even approximately fulfilled. The truth is that there are *no* pieces of the *Beiwerk* which can be fitly

described as headings, with the exception of those consisting of the formula: "These are the generations of" (xxv. 19, xxxvii. 2); and this has always been recognized as the heading to different sections of P. But this heading occurs no fewer than four times (xxv. 12, xxxvi. 1, 9, xxxvii. 2) where no Seder begins!

(3) Even on Dahse's analysis, "K" does not simply emphasize points in the earlier record, but introduces here and there new matter, and maintains throughout an independent view of the national tradition.

(4) It follows from (1) that the question of the original continuity of the P-sections remains exactly as before. Dahse has not succeeded either in extending or in contracting the limits of the document. The only point that remains to be considered is whether the fragments are inserted at the divisions of the Synagogue lectionary.

(5) Here there is one allowance which must be made in justice to Dahse's point of view. He supposes the "K"-passages to have been originally written on the margin of a codex, or at least in some way distinguished from the body of the text (p. 162). He must therefore postulate a subsequent redaction, by which the *Beiwerk* was incorporated in the text; and he is entitled to assume that in this process the *ipsissima verba* of the rubrics, etc., may have been mutilated or effaced. Chap. xlvii. 5–11 shows that this is a very probable occurrence. Hence we cannot require a perfect correspondence between the theory and the facts of the present text. If the theory holds good in a sufficient number of cases to exclude the hypothesis of

accidental coincidence, Dahse can rightly claim that the exceptions are due to later manipulation of the text. We will now consider whether a sufficient number is made out.

(6) In Gen. xii.–l. there are 35 Sedarim and an almost equal number (33) of P-sections, larger or smaller; on an average therefore one P-passage to each Seder. How are these distributed? We find that (excluding chap. xvii., which forms a complete Seder by itself) *nine* occur at or near the beginning of a Seder, *seven* at or near the end, and *sixteen* in other places. And again, that of the 35 Sedarim *ten* have P near the beginning (Nos. 13, 14, 18, 23, 28, 31, 32, 33, 42, 43), *seven* at the end (13, 14, 19, 22, 23, 25, 40); therefore *three* (13, 14, 23) both at the beginning and the end, and *fourteen* (13, 14, 18, 19, 22, 23, 25, 28, 31, 32, 33, 40, 42, 43) either at beginning or end. On the other hand, *eight* (9, 10, 16, 26, 27, 29, 37, 41) have P only in the middle, and *thirteen* contain no trace of P at all (11, 12, 15, 17, 20, 21, 24, 30, 34, 35, 36, 38, 39). Now, I give my opinion with all diffidence; but I confess I cannot see that these statistics (showing 60 per cent. of absolute failures!) are favourable to a theory which asserts that the P-passages were composed with a special regard to the division into Sedarim. Dahse, no doubt, by very violent criticism contrives to add some half-dozen cases where his "K" closes (or opens?) a section: these I decline to accept for reasons already given; but even if they were accepted his case would not be greatly strengthened. At all events we are bound to see if a more adequate solution cannot be found.

(7) While the facts are insufficient to bear out

Dahse's hypothesis, it would appear that the number of coincidences between the P-sections and the Seder-divisions is greater than can be accounted for by the doctrine of chances. In mere bulk about one-seventh of the text of Genesis xii.–l. belongs to P, whereas a P-passage opens or closes about two Sedarim out of five. This fact calls for attention, and it is Dahse's merit to have forced it to the front. Now it is abundantly clear for one thing that in the great majority of cases the Seder-division marks a new start in the narrative. This is perfectly manifest, e.g., at the beginning of chap. xii., xiv., xv., xvi., xvii., xviii., xx., xxi., xxii., xxiv., xxv., xxvii., xxxvii., xxxviii., xxxix., xl., xlviii., xlix., also at xxv. 19, xxviii. 10, xxxii. 4, xxxv. 9, and perhaps some other places. It will be found that, with perhaps two exceptions (xxx. 22, xxxiii. 18), all the Sedarim introduced or closed by P belong to this class: on the other hand divisions which violently interrupt the narrative (such as xxiv. 42, xxvii. 28, xxxi. 3, xli. 38, xliii. 14) are *never* marked by P. Let us suppose, then, that the arrangement of the lectionary is much later than the final redaction of the Pentateuch: the only fact that remains to be explained is the frequent occurrence of P-sections at pauses in the narrative. But that is surely the most natural thing in the world. The redactor who so skilfully dove-tailed P into the connection of JE naturally looked for the interstices of the old narrative as the places where he could most suitably insert the bulk of the new material (see especially chap. xvii., xxiii., xxv. 7 ff., xxvi. 34 f., xxvii. 46–xxviii. 9, xxxv. 9–13, xxxvi.,

xxxvii. 1 f., xlvi. 6–27, xlix. 1*a* and 28*b*–33). And not less naturally, the framers of the lectionary frequently selected these same points as the places where a reading might in most cases fittingly close, just as nearly half of the modern chapter-divisions coincide with divisions of the old Jewish lectionary. The correspondence (such as it is) is amply accounted for by the two processes being guided by parallel aims, and we can understand how the authors of the Sedarim-division would find so often a suitable ending sometimes at the beginning, sometimes at the close, and sometimes in the middle, of a section from P. That seems to me a much more probable and adequate solution than Dahse's of a problem to which he has rightly called attention, but which he has obscured by an excess of learning and perverse ingenuity.*

* The first eight Sedarim (chap. i.–xi.) are not included in Dahse's survey. He merely (p. 152) states his opinion that "K's" hand is recognizable at the head of each Seder (ii. 4, iii. 22, v. 1, vi. 9, viii. 1, viii. 15, ix. 18); and promises a fuller examination of these important chapters in a future publication. Some idea of the line he will take may be obtained from the *Wie erklärt sich*, etc.?, p. 16 f. We there find that iii. 22 is a heading to Seder 3, because it marks no progress in the narration (!!), but contains a "reflection" based on iii. 5, ii. 9; that v. 1, 2, are a recapitulation of chap. i.; vi. 9–12 of v. 32–vi. 8; and ix. 18 f. of something that has gone before. He does not explain on what principle he treats viii. 1, 15 as headings or recapitulations: they stand on the same level of unsuitability as xxx. 22*a* and xxxi. 3. Thus there are four (Nos. 1, 3, 6, 7) of the eight Sedarim which have nothing that by any stretch of courtesy can be called headings; and in three (2, 4, 5) of the rest the heading contains the formula אלה תולדת.—It is of some interest, however, to see how the case stands as regards the critical P.

Dahse in his Reply (p. 508) advances a crowning proof of the correctness of his hypothesis, on which it is necessary to say a few words. It rests on a new exegesis of Nehemiah viii. 8, for which he is indebted to Sellin. In our Revised Version that verse reads thus: "And they read in the book, in the Law of God, *distinctly* (mg. *with an interpretation*); and they gave the sense, so that they understood (mg. and caused them to understand) the reading." Sellin renders, "And they read out of the book of the Law of God, *in sections*, and with explanations (מְפֹרָשׁ וְשׂוֹם שֶׂכֶל), so that they understood what was read"; adding, "Or, according to the LXX even, 'And Ezra made sections and explained.'" He describes this as an "almost staggering argument" for the truth of the pericope-hypothesis. Well, one has heard of staggering arguments derived from Nehemiah viii. 8 before now. It is a verse which

Since more than half of these eleven chapters is assigned to P, it is not surprising that twice (viii. 1, 15) a Seder-division happens to fall in the *middle* of a P-section; and ix. 18 does not belong to P at all. Thus the *only* places where P starts with a new Seder are ii. 4, v. 1, vi. 9; and in each case it is with אלה תולדת. But this formula also occurs three times where there is no Seder-beginning, x. 1, xi. 10, xi. 27. The last instance is enough of itself to condemn the whole Pericope-hypothesis. For this formula is quite as characteristic of Dahse's "K" as it is of P; and if "K" had been the author of the scheme of lessons, he would certainly have made a new Seder commence at xi. 27 instead of xii. 1. We come back to the only tenable position, that the phrase "These are the generations of" is the heading of certain sections of the Priestly Code, with which the framers of the lectionary *sometimes* found it convenient to commence a new lesson.

certain mediaeval Jewish writers cited as evidence that the Massoretic vowels and accents were introduced by Ezra! And I doubt if Sellin's exegesis is much superior to theirs. It turns, as will be seen, on the difficult word מְפֹרָשׁ which Sellin appears to take as a denominative from פרשה: "מפרש, *i.e.* in Parashiyyoth," is his comment. Now (1) even if this exegesis were sound (which I shall show to be highly problematical) we should still lack proof that the sections in which the Law was read were the long Parashas of the Annual Cycle. The name *Parasha* was also given to the shortest paragraphs into which the books of scripture were divided; and these would obviously be more suitable for the purpose of a running popular commentary than the unwieldy Parashiyyoth of the Synagogue lectionary. Moreover, if the latter were meant, not merely the Seder-division, but the Parasha-division as well, would be as old as the time of Ezra, and so Dahse's whole theory of the development of the pericope-system would be knocked on the head. But (2) there is no real ground for supposing that מפרש has anything to do with divisions of the Law at all. It is true that some lexicographers (Siegfried, Buhl, the latter with a "perhaps") and Bertholet in his *Commentary* have suggested the rendering *Abschnittweise* for Nehemiah viii. 8; but König in his *Wörterbuch* rightly remarks that this has no support in the other usage of the word. I am not aware that the use of *Parasha* for a section of the Law can be traced further back than the Mishna; and it is certain that in biblical Hebrew neither the noun nor the verb goes beyond the idea of

*precise definition.** It is extremely hazardous to depart from the contemporary usage of the verb, and give it a technical significance which we do not know that it *ever* acquired. But the verse is textually unsound, and untranslatable as it stands. מְפֹרָשׁ is a passive participle having nothing to agree with in the Hebrew; and to take it (with most) as an adverbial accusative is questionable syntax and at best a poor makeshift. The obvious emendation is to insert "Ezra" with the LXX, reading ועזרא מְפָרֵשׁ ושום שכל; † and rendering "they read in the book of the Law of God, while Ezra *expounded clearly*, giving the sense, so that they understood what was read." That is an interpretation which satisfies every requirement of grammar, etymology and usage, and ought to commend itself to Dahse because of its agreement with the LXX. But even if, with Sellin, we were to render "while Ezra made sections," the circumstantial clause would still denote something which

* Est. iv. 7, פרשׁת הכסף, "the *exact sum* of money"; x. 2, פרשׁת גדלת מרדכי "the *exact account* of Mordechai's greatness." Similarly the verb, in the only two *certain* cases in which it occurs: Numbers xv. 34, "it had not been *distinctly declared* (פֹּרַשׁ) what should be done to him"; Leviticus xxiv. 12, "till it should be *distinctly declared* (לְפָרֹשׁ) by the mouth of the Lord." The reference in both cases is to decision by the sacred oracle, for which in Assyrian the same word is used. So also in biblical Aram.: Ezra iv. 18, מְפָרַשׁ, "made plain." The LXX renders in Nehemiah viii. 8 *ἐδίδασκεν*; the *διέστελλεν* which Sellin evidently has in mind seems to answer to שׂוֹם. But if it is a duplicate rendering of מפרשׁ, the translator must have read the verb פרשׁ (as Psalm lxviii. 14) and not פרשׁ.

† So Haupt, *SBOT*, *Numbers*, p. 51.

Ezra did on the spot and in public assembly, not an intricate literary operation performed previously.

The other historical arguments by which Dahse seeks to identify the "Kompilator" with Ezra are of little account. The documentary theory has no interest in denying that Ezra may have been the author of the Priestly Code, or the redactor who combined P with the rest of the Pentateuch: both views have in fact been held by advocates of that theory. The real question is not whether Ezra was the author of this stratum of the Pentateuch, but whether it was he who compiled the lectionary of the Triennial Cycle, and furnished it with headings for the Sedarim. And that, I submit in conclusion, neither Dahse nor Sellin has made in the least degree probable.

## VIII

# LAST WORDS WITH DAHSE

IN the *Expositor* for December, 1913, Dahse has published a rejoinder to the series of articles now collected in this volume. He does not profess to offer a complete answer to my strictures on his position; and, indeed, in a reply limited to thirty pages that was hardly to be expected. What he has proposed to do is to set forth the result of certain admissions which he supposes me to have made as to the transmission of the names for God in Genesis, to explain why he objects to my treatment of the LXX, and to show the bearing of the whole discussion on the development of Pentateuchal criticism (p. 482). I believe that in regard to all these points my positions are already sufficiently clear to those who may have read my articles with an unprejudiced desire to grasp the real issues of the controversy; and if I had to do only with readers of that class it would be unnecessary for me to add anything to what I have written. But when I find the Dean of Canterbury calling the attention of his friends of the Victoria Institute * to the fact that a "leading English critic"—myself, to wit!—has "thought it necessary" to reply to an opponent

* In a paper read June 5, 1913.

(Dahse) whose learning and ability he, the English critic, is constrained to acknowledge; and straightway drawing the inference that things must be in a bad way with the Higher Criticism—when I see this, I am made to realize that I have another class of readers, who are not at all interested in the scholarly matters under discussion, or the weight of the arguments advanced on either side, but simply enjoy the spectacle of a Higher Critic fighting, as it pleases them to suppose, for his altars and his gods. And for their sakes I have thought it desirable to do something to prevent its being said that the "leading critic" has been reduced to ignominious silence by thirty pages of the *Expositor*. I will proceed, then, with an eye on that gallery, to consider how far Dahse has succeeded in demolishing my argument by pursuing those three aims of his. I, too, disclaim the intention of dealing with every point he has raised; but I shall deal with those that seem essential. I shall have occasion to show that his dialectic rests very largely on misapprehensions of my position which are to me incomprehensible. At times the travesty of my opinions is so grotesque that when I first read it I began to wonder if I had been writing in my sleep. On re-examination I am more inclined to think that Dahse himself was half asleep when he read my articles. I readily acquit him of deliberate intent to mislead; and perhaps my language has not been always as carefully guarded as it might have been: nevertheless, the case is such that I must beg my more serious-minded readers not to trust any statement of my position that appears in Dahse's pages until

they have read, or re-read, the passage in its original connexion.

Now the points in Dahse's reply which seem to me most worthy of notice can be brought under the following three heads. He charges me *first* with having made admissions as to the uncertainty of the transmission of the divine names so extensive that they entirely invalidate my contention that in them we have a reliable, though not infallibly accurate, clue to the analysis of sources; *second*, with having expressed contradictory and mutually destructive opinions about the general soundness of the Massoretic text; and *thirdly*, with entertaining confused and antiquated ideas regarding the use and value of the LXX in textual criticism. I will take up these three points in their order (which is also the ascending order of their importance), reserving for another place a notice of some other arguments and objections which are not susceptible of classification under distinct heads.

1. Let us look first at the admissions which I am alleged to have made as to the uncertainty of the Hebrew divine names in individual cases. I read on p. 484 of the Reply that I have made the sweeping admission that in Gen. i.–x. "the names of God are of little importance for the separation into sources." This refers, I suppose, to the note on p. 164 f., where I have withdrawn a previously published statement, and allowed that in chap. ii.–x. the discrepancies between MT and LXX are so numerous that *if* all those readings in which they do not agree were to be ruled out of action, there would not remain enough names

to be of any use in the analysis. That *hypothetical* admission I was bound in honesty to make as soon as I realized how the land lay. But I have never made the *practical* admission that only those names can be used for analysis which are authenticated by the consent of LXX with MT. On the contrary, I have argued (p. 178 ff.) that in these very chapters (especially in vi.–ix.) an analysis starting from the divine names of the MT is so strikingly confirmed by a great variety of other criteria as to furnish a very high guarantee of the accuracy of the MT in its transmission of these names. Anyhow that is not the kind of admission on which Dahse professes to found his present argument. He was to take only those isolated variants where uncertainty could be proved either from "Eastern" witnesses to which I am supposed to attach special importance, or from the unanimous testimony of the LXX if supported by *any* "Eastern" witness. Of such cases, in chap. i.–x., he cites only three (vii. 1, 9, viii. 15). But as Dahse does not follow up the result of these admissions we must pass them by and go on with him to chap. xi.–l. Here it would appear that I have admitted 16 cases where a name of the MT is doubtful. Dahse instances xiii. 10*a*, *b*, xiv. 22, xv. 6, xvi. 11, xviii. 27, 31, xix. 29*a*, xx. 4, xxviii. 20, xxx. 24, 27, xxxii. 9; in all 13: where the other three are I do not know, unless he has inadvertently counted in vii. 1, 9, viii. 15. Now, strictly speaking, of these only three (xiv. 22, xxviii. 20, xxxii. 9) are admissions of mine; the rest are only admissions which Dahse thinks (in most cases wrongly!) that I am bound to allow

in accordance with general observations which I have made. From that point of view I can only thank him for his moderation: he might easily on his methods have burdened me with a much greater number. Only he should have pointed out that in three of the cases cited (xviii. 27, 31, xx. 4) the alternative is between *Yahwe* and *Adonai*, and has no influence on the analysis. On the other hand I must grant him xvii. 1 and xxi. 1 (*Yahwe* in P) where the name, though not in my opinion *textually* doubtful, is at variance with the division of sources by which the current documentary theory holds. The main thing is that Dahse considers 16 exceptions enough for his argument, and for my part I am not disposed to quarrel about two or three more or fewer. What, now, is his conclusion? It is, once more, that "the use of the divine names in MT can be *of little importance* for the division into sources" (p. 484). Really? Although there are some 210 names in these 39 chapters? I should have thought that a text in which only one divine name in thirteen was doubtful or wrong offered a very encouraging field for the application of Astruc's criterion. But this is just one illustration of the seemingly insurmountable barrier between Dahse's reasoning faculties and mine. And another immediately follows. Dahse regards the fact that *nine* uncertainties (for he *does* exclude xiv. 22, xviii. 27, 31 and xxxii. 9 as not affecting the analysis) are spread over *seven* chapters out of 40 (not in nearly *every* chapter, as Dahse asserts!) as an additional drawback to my way of looking at the matter. To my mind, on the contrary, that

is precisely what saves the situation. If all occurred in one chapter, I should have to admit that so far as that chapter was concerned the divine names were useless as a critical clue. But it is surely obvious to common sense that when they are scattered over many chapters the uncertainty is neutralized, and the risk of error reduced to a minimum, by the immensely larger number of uncontested names in the midst of which they occur.

The following paragraph opens (p. 485) with a very curious specimen of confused reasoning. I maintain, Dahse says, that "we have them (the names) 'in a sufficient number.' And these are to be found in the parallel narratives." I really do not know what idea Dahse meant to convey by these sentences. Certainly I have written, in a particular connexion (p. 185), the words which he proceeds to quote: "The important point is that the book of Genesis contains examples of dual narration, and that the names J and E closely follow the line of cleavage marked out by the parallelism"; and of course that implies that J and E do occur in these narratives "in a sufficient number"; (there are 27 in the passages in question). But that the sufficient number occurs *only* in the parallel passages, or that those which occur elsewhere have no significance for documentary analysis, is a view which I do not think Dahse can possibly mean to attribute to me. I simply do not understand, and must pass on. A little lower down we read: "Now the extraordinary thing is that precisely these chapters of Genesis (i.e., xii. 10–20 ‖ xx. and xxvi. 7–11; xvi. ‖ xxi. 8–21;

xxi. 22–31 || xxvi. 26–33) are among those in which we find Hebrew variants or uncertainties *admitted by Skinner.*"—I remark in passing that *I* have never admitted these variants; and if the reader will look back to the list of uncertain names attributed to me, he will see that xvi. 11 is the only one which Dahse maintains that I *ought* to have admitted. I do not even admit that in xii. 17 ὁ θεός was the original reading of the LXX;* or that in xxi. 17*b* the *Elohim* of MT (and all other witnesses) is rendered doubtful by six Greek cursives; or finally that in xxvi. 29 the θεοῦ of **egj** discredits the *Yahwe* which is read by MT (and all other witnesses).—But supposing all these, and also xvi. 11, to be wrong in MT, how does it affect my argument in the passage with which Dahse is dealing? Let us hear his conclusion on the whole matter (p. 486): "So that it is just in the vaunted parallel narratives that the use of the Divine Appellations fails to act as a guide for attaching the narratives in question to supposed sources." Well, I might reply that it is just in the vaunted parallel narratives that we are not particularly dependent upon them. For the parallelism of the narratives shows that two sources are in evidence, and there are enough unquestioned names (23) to show that one predominantly uses J and the other E; and if even so many as four names were *prima facie* uncer-

* Even if there were evidence (which there is not) that the κύριος which precedes ὁ θεός in certain MSS. was a Hexaplaric addition, that would only prove that ὁ θεός was the reading of Origen's LXX text; and against it we have the Old Latin, with **bw**, **ej**, *al.*, in favour of an original κύριος.

tain, a strong presumption would still remain that the text whose readings correspond with the division of sources has preserved the names of the original documents. The plain truth is that Dahse has taken no pains to understand the drift of the argument he is criticizing. I am not at this point attempting a direct proof that the names of the MT are correct; but I am arguing inversely that a division of sources already established by a variety of indications (among which is parallelism of narration) guarantees the accuracy of the MT in cases which might otherwise be doubtful, by its perfect correspondence with the lines of division marked out by the broader literary analysis. I think that most people will recognize in this a legitimate case of the verification of a working hypothesis by its results. But if Dahse still refuses to distinguish between this kind of argument and a *circulus vitiosus* (see p. 177 above), I have nothing more to say to him.

2. I come next to Dahse's attempts to prove that I stultify myself and give away my whole case by a vacillating estimate of the value of the MT. "In his *Expositor* articles Dr. Skinner still defends the principle of the Hebraica veritas"—that is absolutely untrue!—"but it is otherwise in his commentary on Genesis" (Reply, p. 499). And within these *Expositor* articles I maintain on the one hand that "the Hebrew text possesses credentials to which no version, and perhaps the LXX least of all, can pretend," that "the MT is a solid and sufficient working basis for the literary analysis of the Pentateuch" (Reply, p. 482), that "the MT has substantial claims to be preferred

to a variant of the LXX in all doubtful cases" (p. 499); but concede, on the other hand, that the MT depends on an archetypal MS. which "contains stereotyped errors and defects, eccentricities," and so on. Now it is quite true that I hold both these positions and I deny that there is any inconsistency between them. What I mean is simply that this archetypal MS., imperfect though it was, nevertheless, *on the whole*, and particularly in regard to the divine names, contained a better text than can be obtained from the LXX without its help.* I have given at great length the considerations on which I base this judgment (pp. 165 f., 173 ff.). I can quite understand that my reasoning has failed to convince Dahse, and that the claim I put forward for the MT should seem to him extravagant. But I think he ought to have reminded his readers that when I speak of the MT I speak not merely of the text of one MS., but of that text as substantially confirmed by the Samaritan Pentateuch, which in my opinion goes back to 330 B.C. I have several times (see p. 122 f.) had occasion to remark on Dahse's persistent neglect of the evidence of the Sam. at crucial points of his argument. I now learn (Reply, p. 509) that in his opinion "It is not at present opportune to investigate in detail the mutual relations of LXX, MT, and Sam.," because we may expect a critical edition of the latter from von Gall within the next few months. I agree (as I have said, p. 135) that it is premature in the circumstances to form a final judgment on this question. The critical edition may contain

* See NOTE VI, p. 274.

surprises for all of us, and it is becoming in all of us to adopt an attitude of judicial reserve. But without investigating those relations "in detail," I should have thought it incumbent on a textual critic to have some clear conception of the general bearing of the Sam. on the antiquity of the MT, or at least to keep steadily in mind the possibility that his whole system may tumble into ruin through the commonly accepted date of the Sam. turning out to be right. And whatever be the duty of a textual critic, it is reprehensible in a controversialist to leave out of account an essential element in his opponent's case, and to charge him with contradictions and extravagances which have no existence from his point of view.

This ignoring of the Sam. is again responsible for the pointless polemic which we read on p. 487 of the Reply. Dahse says I admit "that there were *periods* when little care was taken in the transmission of the divine names"—he means in the Hebrew text. I have admitted (p. 173 f.) that there was *one* period (seventh to fifth century B.C.) as to which we cannot have the assurance that the names were transmitted under all the safeguards that came into operation after the canonization of the Law; I am not aware that I have anywhere admitted more than this. Dahse, however, concealing the fact that in the passage to which he refers I am speaking only of the Pentateuchal text, manages to drag in an allusion to "the indiscriminate use (by the Chronicler) of the divine names in the third century," which I have expressly stated to have "had no

effect on the text of the Pentateuch." What was in his mind when he penned the rest of this page it is difficult to make out. "Eastern witnesses," we are told, prove that the "Hebrew testimony" has not been so faithful in the transmission of the names as I suppose. Are there any "Eastern witnesses" older than the Sam.? And what have we to do here with Kennicott's incompletely collated Hebrew MSS.? Or with Nestle's emphasis on the uncertainties of printed texts (which, by the way, has nothing to do with the Pentateuch)? Does Dahse not see that my whole contention turns on the agreement between the Sam. and the MT? If the Sam. be the negligible quantity which he appears to assume, then *of course* my argument falls to the ground; and there was no need of all this display of irrelevant erudition to demolish it. But if the antiquity of the Sam. be a fact, then I conceive that my argument is unanswerable.

3. This brings me to the last point—a question of more than merely controversial importance, viz., my conception of the place and value of the LXX in textual criticism.

Let me first of all clear up certain ambiguities of expression on my part, of which Dahse makes a somewhat unchivalrous, not to say unscrupulous, use. On p. 59 I have spoken of "the assumed Hebrew basis of the LXX, whose existence I have shown to be highly problematical." That sentence, detached from its context, Dahse quotes on p. 498 f. as proof that in substance I deny "that the original LXX has any value at all for the restoration of the original text." Now Dahse

knows very well that I never meant to deny that there was a Hebrew basis to the LXX, or that that basis differed from the MT, or that in certain cases it contained a text superior to the MT. What I did deny, as the context plainly shows, is the existence of a Hebrew basis such as he claims to have discovered, viz., a recension of the Hebrew text in which the divine names had been manipulated on principles regulated by the divisions of the Synagogue lectionary. To that I adhere. Then again I speak (inaccurately of course) of the LXX as "dependent on the MT." What I should have said, to avoid all possible misunderstanding, is: "dependent, so far as the divine names are concerned, on a Hebrew text which, so far as the divine names are concerned, is now correctly represented by the MT." Once more, Dahse takes me to task for referring to "the *text* of the Cambridge Septuagint as the standard text." He marvels that anybody should express such a view "at this time of day." The sense in which I speak of a "standard text" is, I think, sufficiently explained in the context from which he quotes (p. 67), where I say that "the (Cambridge) edition affords a *convenient standard of primary reference* in all comparisons of the various types of text," and where I expressly disavow the idea that it is "necessarily the best witness to the original text." In short, I use it as representing sufficiently for the purpose in hand the vulgar text of the LXX. What more does Dahse want? Does he want to be reminded that it is his own slipshod habit of not isolating readings characteristic of a particular recension from the

vulgar text of the LXX that drives one to appeal to a standard of reference?

But underneath all this purely verbal quibbling there lies, I am now convinced, a real and vital opposition of view regarding the character of the LXX as a whole and the state of its text; and I desire to state this difference with all possible fairness and moderation. As regards myself, then, I have to avow that my conception of the LXX is governed by two leading presuppositions. (1) In the first place I have always held that the Greek version *of the Pentateuch* (note the restriction!) *

* In the case of the historical books there is reason to suspect that several more or less independent Greek translations were current side by side, and that their readings have been combined in MSS. of the LXX (see Moore, *Judges*, *ICC*, p. xliv; Smith, *Samuel*, p. 402). These sometimes represent different renderings of the same original; but considering the fluid condition of the Hebrew text of these books at the time when the translations may have been made, it would not be in the least surprising to find (assuming the translations to have been independent of one another) that they follow divergent Hebrew exemplars. For the books of Kings, on the other hand, Rahlfs appears to reject absolutely the hypothesis of separate Greek versions, and maintains the dependence of Lucian on the original LXX so far as his Greek basis is concerned (*Sept.-stud.*, 3, p. 171). But the point here to be insisted on is that the LXX version of the Pentateuch stands on an entirely different footing from the Greek translations of the historical books. The latter belong, with the prophetic writings, to the second division of the Jewish Canon; and it is quite probable that they had already been translated before a particular recension of them had been officially canonized. Whereas the Pentateuch had been the acknowledged Sacred Code of Judaism for a century and a half before it was turned into Greek, and although it existed in slightly different recensions, there is no evidence whatever that more than one of these recen-

originated in a single translation made once for all (except perhaps Exod. xxxv.–xl.) in the third century B.C., and propagated in Greek MSS. without (so far as we know) systematic revision till the third century A.D. Sporadic emendation in accordance with the Hebrew must have taken place before the latter date; and it is *conceivable* that the Hebrew text employed for this purpose may have differed both from the MT and from the Hebrew of the original LXX. Whether any such cases can be proved in the Pentateuch I do not venture to say. But apart from such rare and hypothetical cases, it follows from what I have said that wherever a recension or MS. of the LXX presents an undoubted *Hebrew* variant from MT, that must be accepted as the reading of the Hebrew text on which the LXX as a whole is based. I consider it therefore an error in method, whenever an interesting variant is found in the LXX text, to fly at once to the assumption of a special Hebrew recension, without at least exhausting the possibility that—if it be a Hebrew variant at all—it is the Hebrew of the original LXX. And that is what Dahse constantly does.

That there is some confusion in Dahse's mind at this point appears from his citing (p. 489)

sions was ever translated into Greek. It is obvious, therefore, that no inference from the analogy of any of the historical or prophetical books holds good for the Pentateuch. Dahse himself often appeals to the sound principle that each book must be considered by itself; and until definite proof is forthcoming that different Hebrew recensions of the Law were produced in Greek we must adhere to the position that all the diversities in the LXX text of the Pentateuch go back to the original translation, accommodations to the existing Hebrew always excepted.

*against me* an opinion of Kittel which, if I mistake not, supports my view. Kittel says that as early as the end of the fourth and beginning of the third century B.C. there were already groups or families of Hebrew MSS., each providing "an independent and characteristic presentment of the MS. tradition." There he seems to me to go somewhat beyond the actual evidence; but that is not the point. He goes on to say: "We learn to recognize two of these families in the MT and the LXX"; and again, "the LXX presents an independent recension of the Hebrew text." That is to say, the MT is one recension, and the LXX represents another. Now that is exactly what I hold, and what I have said almost in so many words on pp. 114, 132. But according to Dahse the LXX represents not one Hebrew recension but a great many. And a further question arises: In what way does it represent them? I have already put that question to Dahse in the June *Expositor* (p. 508: p. 77 above) where I wrote: "What is meant when it is said that the recension 'goes back' to a Hebrew original?" and I went on to say that Dahse could not possibly mean that it is a fresh translation from the Hebrew. Is that after all what he does think? He writes (Reply, p. 495) that his "conception of *separate Hebrew originals* is not a too facile or improbable assumption." That is still ambiguous. But if he does not mean fresh translation, but only systematic correction after a Hebrew text, I reply that all the data he has hitherto published are insufficient to show the use for that purpose of any Hebrew text except the MT. And that brings me to the second point.

(2) The second factor which determines my attitude to the LXX problem is the conviction that the Hebrew text employed by Origen and Lucian for the correction of the LXX was the MT or an earlier Greek version which followed MT. As to Origen this, I suppose, has never been questioned save by Dahse. Nor, so far as I am aware, does any other opinion prevail among authoritative scholars in regard to Lucian. Procksch, for example, whom Dahse strangely cites as opposing this view (p. 495), plainly holds it; for the whole context of the section from which the extract is taken shows unmistakably that for him "the Hebrew text" is simply the Massoretic. It is true that Driver (*Samuel*, p. xlix) leaves it an open *possibility* that Lucianic readings which are self-evidently superior to the existing MT "may be based directly upon Hebrew MSS., which had preserved the genuine reading intact"; but there again we must bear in mind that even if this should be the case as regards the books of Samuel, the fact affords no presumption that the same explanation can be applied to the Pentateuch. On the other hand, we find that Rahlfs, in an exhaustive examination of Lucianic readings in the books of Kings (*op. cit.*, pp. 170–191), not only refrains (as we have seen) from tracing any of them to an independent Hebrew recension, but when he mentions (p. 185 f.) the suggestion that Lucian might have corrected after a Hebrew text different from MT, puts it aside as "wenig wahrscheinlich," on the ground that the Hebrew text was already "very constant" in the second or third century after Christ. That seems to me

to mean that one of the foremost living authorities on the LXX finds so little indication of Lucian's having known any other Hebrew text than MT, that when he comes to a particular difficulty which might be solved by that theory he refuses to entertain the suggestion.

Dahse of course dissents *in toto* from this opinion; but he has adduced nothing whatever to make his view acceptable. Even the speculation of Hoberg to which he refers on p. 495—that Lucian may have consulted the Targum of Onkelos—if there were anything in it, would but strengthen the presumption that his only Hebrew authority was the MT. The τοῖς Ἑβραικοῖς of the well-known passage of the Pseudo-Athanasius has never yet been taken to refer to anything but the MT; and I cannot imagine how Dahse could suppose that he found support for his view either there or in the sentence from Suidas which immediately precedes. The Lucianic text of the Pentateuch has never yet been thoroughly investigated; but when Dahse can produce a few unambiguous instances *in the Pentateuch* where a reading of any recension of the LXX goes back to a Hebrew original differing *both* from MT *and* from the basis of the LXX, I shall acknowledge that my views of the LXX are untenable. Meanwhile, I certainly do not stand alone when I say that nothing short of the most searching and comprehensive induction—such as Dahse has not yet attempted—will suffice to establish a dependence of Origen or Lucian on another Hebrew than the MT.

Now I am not putting forward these views at present as the absolute and incontestable truth

about the LXX. I may be wrong, and Dahse *contra mundum* may be right. But I do say that they are not the obsolete and old-fashioned and ridiculous notions that Dahse would fain have his readers believe. They are in accordance with all that I know of the facts of the case, and of the practice of the best workers in the field of LXX criticism. I put them forward here above all as representing a consistent critical point of view which has regulated all my effort and thinking in this department of exegesis. I hope the candid reader will perceive that when in my commentary on Genesis I made extensive use of LXX readings, I was not, as Dahse insinuates, simply registering "scribal and accidental errors," but was citing possible, or probable, or certain, indications (as the case might be) of the true original Hebrew text.

So much, then, for our respective estimates of the LXX in general. There remains the question of the value of the two recensions **egj** and **fir**, which Dahse considers specially important in relation to the problem of the divine names in Genesis. Now here I would gladly retract anything I have said on pp. 60 ff., or elsewhere, that can justly be charged with minimizing the importance of these recensions. All that I have there said in my zeal for popular explanation is, indeed, perfectly true. Agreement of a group of MSS. in characteristic readings does not of itself prove systematic revision; and systematic revision does not prove that the group represents one of the historical recensions. Dahse himself must allow the existence of sub-recensions; for he

recognizes (besides the Hexaplaric family) no fewer than four important groups in Genesis; and only two at most can be the work of Lucian and Hesychius. I learn, moreover, from Dahse (p. 494) that Wiener has since actually characterized **egj** as a "sub-recension on the basis of a late Lucianic text." When Dahse now furnishes me (p. 492) with a list of five passages to show that the groups **fir, egj** and **dnpt** are true recensions I fully assent to his conclusions; and only remark that there is nothing there that militates against what I have said above as to the fundamental relations of the Greek and Hebrew texts. Perhaps it was wrong in me even to hint at the possibility that **fir** could be something less important than Lucian or Hesychius, although I do not yet see why that might not be so, seeing there is such difference of opinion as to which it is! At all events I have never categorically asserted that it is so; and Dahse might have had the fairness to acknowledge that I expressly waived discussion of that point in deference to his superior knowledge; and that I undertook on his own assumptions to inquire (*a*) whether these recensions observe recognizable principles in their treatment of the divine names, and (*b*) whether there is any evidence that they rest on special Hebrew recensions. And I said that the second of these questions was very much more vital than the first. But let us see what Dahse has to say in reply to my objections.

(*a*) In regard to the first question, Dahse does not challenge any statement I have made; but he affirms that with all my polemics I cannot

help admitting that he has hit upon the truth in both cases. That is rather an overstatement of the result of my inquiry. The tendency which Dahse attributed to **egj** is "to use only one and the same name for God in one section." In summing up the evidence I say (p. 74 above) that the recension goes a little, *but only a little*, beyond the ordinary LXX in assimilating a name to those in the immediate context; that I recognize this leaning in *at most* seven cases, while the opposite appears in three; and that the seven cases are adequately explained as *unconscious* adaptations to the nearest divine names. If Dahse is content to take that as admitting the truth of his observation I have no more to say, except that he is remarkably easy to satisfy in the way of admissions.—Again, **fir** is described as an "Elohistic edition of Genesis," and I am said to have admitted the truth of this description on p. 79 f. I cannot argue the whole question over again; but I may observe that he has missed the chief point of my criticism. I asked, as he truly says, how on his hypothesis *κύριος ὁ θεός* appears nine times in **fir** for *ὁ θεός* of the ordinary LXX; and *that* he answers by endeavouring to show by one expedient or another that in such cases *κύριος must* or *may* have been the original reading of the LXX.* But he says that I have asked him how it happens that in this recension *ὁ θεός* stands for *κύριος* nine times, and why we do not find *κύριος*

* Except in xvi. 11, which, as a case where *ὁ θεός* is "demonstrably" (?) original, demands special treatment. Here the explanation offered is that Lucian, finding *ὁ θεός* in his LXX and יהוה in MT, combined the two in *κύριος ὁ θεός*.

ὁ θεός in these instances too; and that also he answers in a way which I will not stay to criticize. For really I should never have dreamed of asking any such thing. It seemed to me that the substitution of ὁ θεός for κύριος was just what we might expect in an "Elohistic edition" of Genesis; and my only difficulty was that it occurred so seldom.* No! my question was this: "why κύριος, *though* changed to ὁ θεός in nine cases, is nevertheless retained alone in no fewer than *thirty-five*." And to that question, which touches the weak spot of his hypothesis, Dahse vouchsafes no reply whatsoever. So I hope his readers will *not* too hastily conclude that I was wrong in saying "that no principle is consistently followed by the author of the recension in his use of the divine names, or, if there be, that Dahse has not detected it." Of course I allow that the 28 cases where κύριος ὁ θεός replaces κύριος count in his favour: only I think that 35 exceptions to his principle are too many.

(*b*) But I have already stated that I do not attach extreme importance to that part of the theory: the vital question is whether the recensions had a special Hebrew basis. And Dahse seems never to have perceived that when the evidence is thoroughly sifted this issue proves to be independent of the former. As regards **egj** Dahse's contention was that there is a group of

* Dahse has slightly shifted his ground here. In his Reply he says the chief characteristic of **fir** is κύριος ὁ θεός. But in his *Textkrit. Mat.*, p. 113 (which I was criticizing), the "Elohistic character" of the recension is seen in the *substitution* of ὁ θεός for κύριος; and this point he never touches in the Reply.

nine Hebrew MSS. (see above, p. 75) with which **egj** is in frequent agreement. Against this I urged that in the first place there is no sufficient evidence that these MSS. form a true group of interdependent documents at all; and further that only in three places does **egj** agree with them: never with more than one MS. at a time; whereas in 29 (there are really 30) cases where **egj** diverges from MT, it has no support from the Hebrew group. Dahse now produces (p. 495 f.) five other cases in which members of the group agree—not with **egj** but—with the Sam. (and occasionally with the Vulgate and Peshitta),* and points out that the Sam. happens *once* (xx. 18) to agree with **ej**. What does that prove? Either that the Sam. is a member of the group, or else nothing at all. I will not spend time in examining the five passages, because I wish to fix attention on the essential fact that in not one of them does **egj** follow the group. So that even if there be a group, and whether Sam belongs to that group or not, **egj** stands outside. And when Dahse asks me if I consider the four agreements of **egj** with the Hebrew group *plus* the Sam. to be "once again fortuitous," I point to the 30 disagreements, and answer that I most assuredly do. And I say further that such evidence seems to me ludicrously inadequate to establish the dependence of **egj** on a special Hebrew original.—As to **fir**, Dahse thinks I am anxious to believe that his theory

* Gen. ii. 12: K9, Sam. Vulg. ? + מאר; v. 32: K9, 80, Sam. Vulg. + נח; vii. 1: K601, 686, Sam. Pesh. יהוה for אלהים; xxvi. 22: K150, Sam. Pesh. ויחפרו pl. for sg.; xxvii. 24: K150, Sam. האתה for אתה.

depends on only one passage (Gen. xlii. 5). Well, it was all he gave! He omits to mention the fact which I pointed out to him, that in that one passage he had got hold not of a Hebrew MS. but of a printed edition of the Talmud. He now adds xxiv. 45, where **fir** *partly* agrees with Sam. Pesh.,* in an addition naturally supplied from v. 17 or v. 43; and points rather timidly to xviii. 19, xxix. 3, where **fir** is said to show agreements with "Eastern witnesses."† That is all. Thus we are invited on the basis of four dubious agreements to postulate a special Hebrew recension for **fir**. It is not enough. Moreover not one of the four cases produced touches the use of the divine names; so that even if we were to concede the existence of the alleged Hebrew recension, there is not an atom of evidence that its distribution of divine names corresponds at all with what we have in **fir**.‡

I do not think it would serve any good purpose to pursue the discussion further. I have of course omitted a great many details in Dahse's last publication; just as he has refrained from treating some details of my argument to which I attach

* Sam., Pesh. + מעט מים מכדך : **fir** + *μικρὸν ὕδωρ*.

† I find that in xxix. 3 Sam. Pesh. read הרעים for העדרים of MT, and **ir** (with the OL and three other versions) have *ποιμένες* for *ποίμνια* of LXX. But since in *v.* 8 the whole LXX agrees with Sam. in reading רעים (*ποιμένας*), surely the more natural explanation is that in both verses רעים was the reading of the common original of Sam. and LXX, and that in *v.* 3 it was afterwards assimilated in the ordinary LXX to MT.—In xviii. 19 MT has ידעתיו, LXX *ἤδειν* (= ידעתי Sam.) **fir** *ἤδει* (= ידע?). Where is the agreement?

‡ See NOTE IV., p. 271.

the highest importance. He is a diligent "picker up of unconsidered trifles," and I have commented on a good many of these in the Notes to this volume. His general line of argument—irrelevant and inconsequent as I conceive most of it to be—does not easily lend itself to consecutive discussion. I have stated my own position as clearly as I could, and with almost needless fulness, in the preceding pages; and I hope I have now shown that it stands clear of the misconceptions and misrepresentations in which Dahse has sought to invest it. The one great defect of which he complains (p. 501)—my neglect of his Pericope-theory of the so-called Priestly Code in Gen. xii.–l.—is now made good in the supplementary chapter (VII) here published for the first time; a section which was in writing before the Reply appeared. With that, my task is finished. Dahse will doubtless go on his way rejoicing, a mighty conqueror, tilting against windmills, and thrice slaying his slain; but I shall be content to admire his progress from afar. As for the wicked insinuation with which he has thought fit to close his article, it is a solitary lapse on his part from the amenities of public debate, and is best left to the judgment of the charitable reader. I will only say to him that I know nothing of "situations" in this matter. If either he or Wiener had convinced me of material error, I should willingly have acknowledged myself in the wrong. They have not done so; and it is unworthy of a scholar so lightly to impute a dishonourable motive to an opponent who may have caused him much trouble, but who has hitherto treated him with respect.

# TABLES

## TABLE I

PASSAGES WHERE THE CAMBRIDGE SEPTUAGINT HAS A DIFFERENT DIVINE NAME FROM MT, WITH VARIANTS OF THE LEADING UNCIALS

| | MT. | Camb. LXX. (= Cod. A). | Uncial Variants from Camb. LXX. | Agreeing with— |
|---|---|---|---|---|
| $2^{5}$ | יהוה אלהים | ὁ θεός | | |
| $2^{7}$ | יהוה אלהים | ὁ θεός | $M^{mg}$ = MT | |
| $2^{9}$ | יהוה אלהים | ὁ θεός | | |
| $2^{19}$ | יהוה אלהים | ὁ θεός | | |
| $2^{21}$ | יהוה אלהים | ὁ θεός | | |
| $4^{1}$ | יהוה | τοῦ θεοῦ | | Vulg. ? ὁ Ἑβρ. ὁ Συρ. |
| $4^{4}$ | יהוה | ὁ θεός | | |
| $4^{6}$ | יהוה | κύριος ὁ θεός | | |
| $4^{9}$ | יהוה | ὁ θεός | E κύριος ὁ θεός | |
| $4^{10}$ | — | ὁ θεός | | |
| $4^{15a}$ | יהוה | κύριος ὁ θεός | | |
| $4^{15b}$ | יהוה | κύριος ὁ θεός | | |
| $4^{16}$ | יהוה | τοῦ θεοῦ | | |
| $4^{26}$ | יהוה | κυρίου τοῦ θεοῦ | | |
| $5^{29}$ | יהוה | κύριος ὁ θεός | | |
| $6^{3}$ | יהוה | κύριος ὁ θεός | | |
| $6^{5}$ | יהוה | κύριος ὁ θεός | | |
| $6^{6}$ | יהוה | ὁ θεός | | |
| $6^{7}$ | יהוה | ὁ θεός | | |
| $6^{8}$ | יהוה | κύριος ὁ θεός | | |
| $6^{12}$ | אלהים | κύριος ὁ θεός | | |
| $6^{22}$ | אלהים | κύριος ὁ θεός | | |
| $7^{1}$ | יהוה | κύριος ὁ θεός | | |
| $7^{5}$ | יהוה | κύριος ὁ θεός | | |
| $7^{16b}$ | יהוה | κύριος ὁ θεός | E ὁ θεός | |
| $8^{15}$ | אלהים | κύριος ὁ θεός | | |
| $8^{20}$ | יהוה | τῷ θεῷ | L = MT | |
| $8^{21a}$ | יהוה | κύριος ὁ θεός | E = MT | (*Hex.* κύριος — ὁ θεός.) |
| $8^{21b}$ | יהוה | κύριος ὁ θεός | | (ditto) |

TABLE I—*continued*

| | MT. | Camb. LXX. (=Cod. A). | Uncial Variants from Camb. LXX. | Agreeing with— |
|---|---|---|---|---|
| $9^{12}$ | אלהים | *κύριος ὁ θεός* | *D*=MT | |
| $9^{16}$ | אלהים | *ἐμοῦ* | | |
| $10^{9a}$ | יהוה | *κυρίου τοῦ θεοῦ* | D=MT | |
| $11^{9b}$ | יהוה | *κύριος ὁ θεός* | | |
| $12^{17}$ | יהוה | *ὁ θεός* | E=MT | |
| $13^{10a}$ | יהוה | *τὸν θεόν* | | Pe. אלהים |
| $13^{10b}$ | יהוה | *τοῦ θεοῦ* | | Pe. אלהים |
| $13^{13}$ | יהוה | *τοῦ θεοῦ* | | |
| $13^{14}$ | יהוה | *ὁ θεός* | EM=MT | |
| $14^{22}$ | יהוה | — | | Pesh. om. |
| $15^{6}$ | יהוה | *τῷ θεῷ* | | Pe. Vulg. אלהים |
| $15^{7}$ | יהוה | *ὁ θεός* | | |
| $15^{18}$ | יהוה | *ὁ θεός* (*D*) | M *κύριος ὁ θεός* (A *vacat*) | |
| $16^{5}$ | יהוה | *ὁ θεός* | | |
| $16^{7}$ | יהוה | *κυρίου τοῦ θεοῦ* | | |
| $16^{8}$ | — | *κυρίου* | | |
| $18^{1}$ | יהוה | *ὁ θεός* | | |
| $18^{14}$ | יהוה | *τῷ θεῷ* | | |
| $19^{29a}$ | אלהים | *κύριον* | E om. | K199 יהוה |
| $19^{29c}$ | — | *κύριον* | E *κύριον τὸν θεόν* | |
| $21^{2}$ | אלהים | *κύριος* | | |
| $21^{6}$ | אלהים | *κύριος* | | |
| $24^{40}$ | יהוה | *κύριος ὁ θεός* | *D*MS=MT | |
| $25^{21b}$ | יהוה | *ὁ θεός* | | |
| $28^{13b}$ | יהוה | — | | K103 om. |
| $28^{20b}$ | אלהים | *κύριος ὁ θεός* | E *κύριος* | K193 יהוה |
| $29^{31}$ | יהוה | *κύριος ὁ θεός* | | |
| $30^{24}$ | יהוה | *ὁ θεός* | | Pe. אלהים |
| $30^{27}$ | יהוה | *ὁ θεός* | | Pe. אלהים |
| $31^{44}$ | — | *ὁ θεός* | | |
| $31^{49}$ | יהוה | *ὁ θεός* | | |
| $31^{50}$ | אלהים | — | [The clause found only in Hexaplaric MSS.] | |
| $32^{1}$ | — | *θεοῦ* | | |
| $35^{9b}$ | — | *ὁ θεός* | | Sam. אלהים |
| $38^{7b}$ | יהוה | *ὁ θεός* | | |
| $38^{10a}$ | יהוה | *τοῦ θεοῦ* | | |
| $49^{28}$ | — | *τῷ θεῷ* | F$^{b}$=MT | |
| $50^{24c}$ | — | *ὁ θεός* | | |

## TABLE II

### UNCIAL VARIANTS FROM MT ON PASSAGES NOT INCLUDED IN TABLE I

| | MT. | VARIANT. | AGREEING WITH— |
|---|---|---|---|
| $2^{4}$ | יהוה אלהים | M *ὁ θεός* | |
| $3^{1b}$ | אלהים | E *κύριος ὁ θεός* | K132 יהוה אלהים |
| $3^{8b}$ | יהוה אלהים | E *ὁ θεός* | |
| $3^{9}$ | יהוה אלהים | L *κύριος* | |
| $3^{13}$ | יהוה אלהים | L *ὁ θεός* | |
| $3^{22}$ | יהוה אלהים | $M^{txt}$ *ὁ θεός* | K152 אלהים |
| $4^{3}$ | יהוה | E *ὁ θεός* | |
| $4^{13}$ | יהוה | EM *κύριος ὁ θεός* | |
| $6^{13}$ | אלהים | $D^{sil}$ *κύριος ὁ θεός* | |
| $7^{9}$ | אלהים | M *κύριος ὁ θεός*<br>E *κύριος* | Sam. Vulg. ? K155 יהוה |
| $7^{16a}$ | אלהים | *D*M *κύριος* | |
| $9^{8}$ | אלהים | E *κύριος ὁ θεός* | |
| $9^{17}$ | אלהים | *D κύριος ὁ θεός* | |
| $9^{26}$ | יהוה | L om. | |
| $10^{9b}$ | יהוה | EM *κύριος ὁ θεός* | |
| $11^{8}$ | יהוה | E *κύριος ὁ θεός* | |
| $11^{9a}$ | יהוה | *D κύριος ὁ θεός* | |
| $12^{7}$ | יהוה | M *κύριος ὁ θεός* | |
| $13^{4}$ | יהוה | M *κύριος ὁ θεός* | |
| $15^{2}$ | אדני יהוה | L om., M *δέσποτα* | |
| $18^{22}$ | יהוה | M *κύριος ὁ θεός* | |
| $19^{24a}$ | יהוה | E *κύριος ὁ θεός* | |
| $26^{2}$ | יהוה | EM *ὁ θεός* | |
| $26^{25a}$ | — | E *κύριος* | |
| $28^{4}$ | אלהים | E *κύριος ὁ θεός* | |
| $29^{35}$ | יהוה | E *κύριος ὁ θεός* | |
| $30^{6}$ | אלהים | E *κύριος* | |
| $30^{30}$ | יהוה | E *ὁ θεός* | |
| $35^{1b}$ | אל | EL *(τῷ) κυρίῳ* | |
| $35^{3}$ | אל | E *κυρίῳ* | |
| $35^{10}$ | אלהים | *D* — | |
| $45^{9}$ | אלהים | *D κύριος* | |
| $48^{15b}$ | האלהים | B *ὁ κύριος* | |

# TABLE III

## SHOWING DISTINCTIVE READINGS (OR READINGS GIVEN BY DAHSE AS SUCH) OF DIVINE NAMES IN THE RECENSION egj, WITH THEIR RELATIONS TO OTHER TEXTS

* Passages marked thus are omitted by Dahse without obvious reason.
† Passages marked thus are included by Dahse, but are hardly distinctive of the recension.

| | Camb. LXX. | egj. | MSS. | Agreeing with— | | Remarks. |
|---|---|---|---|---|---|---|
| | | | | LXX Authorities. | Hebrew Authorities. | |
| *2$^{5}$ | *ὁ θεός* | *κύριος ὁ θεός* | ej | bw $c_2$ Arm.-codd. | MT | |
| *2$^{27}$ | *ὁ θεός* | *κύριος ὁ θεός* | ej | $M^{mg}$ $fh^{b}$ (uid) $c_2$ Or-gr | MT | |
| 3$^{1b}$ | *ὁ θεός* | *κύριος ὁ θεός* | ej | $v^{mg}$ Luc. | K132 | |
| 3$^{11}$ | — | *κύριος ὁ θεός* | ej | Sah. Eth. | | |
| *3$^{22}$ | *κύριος ὁ θεός* | *κύριος ὁ θεός* | ej | AE ft svx$c_2$ Arm. Boh. | MT | |
| †4$^{3}$ | *τῷ κυρίῳ* | *τῳ κυρίῳ* | egj | LXX (except Emry Arm. Sah.) | MT | |
| †4$^{13}$ | *τὸν κύριον* | *κύριον τον θεόν* | ej | LXX (except dgptd$_2$ bw afio $c_2$) | | |
| 5$^{29}$ | *κύριος ὁ θεός* | *κύριος ὁ θεός ἡμῶν* | ej | | | |
| †6$^{3}$ | *κύριος ὁ θεός* | *κύριος ὁ θεός* | egj | Entire LXX | | |
| 6$^{13}$ | *ὁ θεός* | *κύριος ὁ θεός* | ej | $D^{sil}$ clmort qu $sv^{mg}$ y $c_2$ Arm. Sah. Eth. Syr., Hex. | | |
| †6$^{22}$ | *κύριος ὁ θεός* | *κύριος ὁ θεός* | e | LXX (except dgnpd$_2$ bw $sv^{txt}$ x fi aklm, Pal. Sah.) | | gj read ὁ θς.=MT |
| †8$^{20}$ | *τῷ θεῷ* | *τῷ θεῷ* | ej | DE chkl qruy$c_2$ and Vers. | | |
| †9$^{17}$ | *ὁ θεός* | *κύριος ὁ θεός* | j | D bw r $s^{mg}$ $v^{mg}$ Arm. | | eg read ὁ θς.=MT |
| †10$^{9A}$ | *κυρίου τοῦ θεοῦ* | *κυρίου τοῦ θεοῦ* | ej | LXX (except D aigoqrc$_2$) | | |
| †10$^{9b}$ | *κυρίου* | *κυρίου τοῦ θεοῦ* | egj | LXX (except $AD^{sil}$ hl y $c_2$ iknr) | | |

| | | | | | | |
|---|---|---|---|---|---|---|
| †11[5] | κύριος | κύριος | egj | LXX (except dpd$_2$bw m) | MT | |
| *11[5] | κύριος | κύριος ὁ θεός | ej | ckmr | | |
| 12[17] | ὁ θεός | κύριος | ej | E bw o qu hlt OL Boh. | MT | |
| 13[4] | κυρίου | κυρίου τοῦ θεοῦ | egj | M dpt f svxc$_2$ Arm. | | |
| 15[4] | κυρίου | θεοῦ | egj | bw rv Arm. Sah. | | |
| 16[7] | κυρίου τοῦ θεοῦ | κυρίου τοῦ θεοῦ | ej | DM hlnoqtuy, etc. | | |
| 18[13] | κύριος | κύριος ὁ θεός | ej | fir d$_2$ | | |
| 18[20] | κύριος | κύριος ὁ θεός | ej[mg] | fir | | |
| 18[22] | κύριος | κύριος ὁ θεός | ej | M s[mg] v[mg] c$_2$[mg] Sah. Pal. | | |
| 18[26] | κύριος | κύριος ὁ θεός | ej | fir s | | |
| †18[27] | τὸν κύριόν | τὸν κύριον | ej | LXX (except dgptmsxd$_2$ fir Boh. Sah.) | K69, 89, 111 132+14R?? | |
| 18[31] | τὸν κύριον | τὸν κύριόν μου | egj | fmsvc$_2$d$_2$* Boh. Sah. | MT | |
| 19[16a] | κύριον | κύριον τὸν θεόν | ej | OL | | |
| †19[29a] | κύριον | κύριον | egj | LXX (except bw fir acm c$_2$ Pal.) | K199 | |
| 19[29b] | ὁ θεός | κύριος | ej | | | |
| 20[4] | κύριε | κύριε ὁ θεός | ej | | | |
| 20[8] | — | τὸν κύριον | ej | fir c$_2$ | | |
| 20[18] | κύριος | ὁ θεός | ej | bw Boh. | Sam. | |
| 21[1a] | κύριος | κύριος ὁ θεός | ej | fr d$_2$ | | |
| †21[1b] | κύριος | κύριος ὁ θεός | j | a d$_2$ | | eg read κς. = MT |
| 21[2] | κύριος | κύριος ὁ θεός | egj | dptd$_2$ fr a s | | |
| 21[4] | ὁ θεός | κύριος ὁ θεός | ej | fr a c ps[a]? Arm. | | |
| 21[6] | κύριος | κύριος ὁ θεός | ej | d$_2$ | | |
| *26[12] | κύριος | ὁ κύριος | eg | M dnp c | MT | |
| *26[22] | κύριος | ὁ κύριος | egj | | MT | |
| *26[24] | κύριος | κύριος ὁ θεός | egj | Eth. | | |
| 26[29] | κυρίου | θεοῦ | egj | | | |
| 28[20b] | κύριος ὁ θεός | κύριος | egj | E. Eth. | K193? | |
| 32[10] | κύριε | κύριε ὁ θεός | ej | kqu Sah. | | |

# TABLE IV

## CHARACTERISTIC DIVINE NAMES OF THE RECENSION fir

| | Camb LXX. | fir. | MSS. | Agreeing with— | | Remarks. |
|---|---|---|---|---|---|---|
| | | | | LXX Authorities. | Other Texts. | |
| *3[13] | κύριος ὁ θεός | ὁ θεός | ir | bw dgpd$_2$ aoxy Boh. Pal. Phil. | | |
| †3[24] | — | κύριος ὁ θεός | fi$^a$ | LXX (except mr Sah.) | Pesh. | |
| 4[3] | τῷ κυρίῳ | τῷ θεῷ | r | Ey Arm. Sah. Phil. | | |
| *4[9] | ὁ θεός | κύριος ὁ θεός | fi$^a$r | E 17 curs. Arm. Boh. Sah. | | |
| 6[8] | κυρίου τοῦ θεοῦ | τοῦ θεοῦ | i*r | bdt | | |
| 6[11] | τοῦ θεοῦ | κυρίου τοῦ θεοῦ | i$^a$r | a j$^e$ Sah. | | f has ὁ θεός: so 6[9] 6[13]: all = LXX |
| 6[12] | κύριος ὁ θεός | ὁ θεός | f | x | MT | |
| 6[22] | κύριος ὁ θεός | ὁ θεός | fi | 11 curs. Pal. | MT | |
| †7[9] | ὁ θεός | κύριος ὁ θεός | f | M kmt Arm. Boh. | | ir ὁ θεός |
| 7[23] | — | ὁ θεός | fi$^a$r | | | |
| *?8[20] | τῷ θεῷ | κυρίῳ τῷ θεῷ | f | n Sah. | | i κυρίῳ: r τ. θεῷ |
| 9[8] | ὁ θεός | κύριος ὁ θεός | f$^a$i$^a$r | E e Arm. Sah. | | |
| †9[17] | ὁ θεός | κύριος ὁ θεός | r | D bwj $^{smg}$ v$^{mg}$ Arm. | | f om. |
| †10[9a] | κυρίου τοῦ θεοῦ | κυριου τοῦ θευῦ | fi | LXX (except agi*oqrc$_2$) | | |
| 10[9b] | κυρίου | τοῦ θεοῦ | ir | kn Phil. | | |
| †11[5] | κύριος | κύριος | fir | Entire LXX | MT | |
| †11[6] | κύριος | κύριος | fi | ditto | MT | |
| †11[8] | κύριος | κύριος | fir | LXX (except E, 12 curs. and Arm.) | MT | |
| 11[9a] | κύριος | κύριος | fr | | MT | |

| | | | | | | |
|---|---|---|---|---|---|---|
| 11$^{9b}$ | κύριος | κύριος | fr | bw ky Phil. | MT | |
| †12$^{1}$ | κύριος | κύριος ὁ θεός | r | M adpd$_2$ | | |
| †12$^{7a}$ | κύριος | κύριος ὁ θεός | r | | | |
| †12$^{7b}$ | κυρίῳ | κυρίῳ | fr | Entire LXX | MT | |
| †12$^{8a}$ | τῷ κυρίῳ | κυρίῳ τῷ θεῷ | r | Sah. | | |
| †12$^{8b}$ | κυρίου | κυρίου τοῦ θεοῦ | r | Sah. | | |
| 12$^{17}$ | ὁ θεός | ὁ θεός | fr | AM ; 8 curs., etc. | | i *vacat* |
| †13$^{10a}$, $^{b}$, $^{13}$ | ὁ θεός | ὁ θεός | fr | Entire LXX | 13$^{10ab}$ = Pesh. | |
| †13$^{18}$ | κυρίῳ | κυρίῳ τῷ θεῷ | r | A*(uid) Arm. Boh. | | f τῷ κυρίῳ |
| †15$^{2}$ | δέσποτα κύριε | δέσποτα | fr | M bw dptd$_2$ egj v clty Boh. Sah., etc. | | |
| 15$^{18}$ | ὁ θεός | ὁ θεός | fr | *D* y | | |
| 16$^{2}$ | κύριος | ὁ θεός | fir | | | |
| †16$^{5}$ | ὁ θεός | ὁ θεός | fir | Entire LXX | | |
| 16$^{7}$ | κυρίου τοῦ θεοῦ | τοῦ θεοῦ | fir | bw dgnp amosvxc$_2$ Arm. Syr.-Hex. Phil. | | 16$^{8}$, $^{9}$, $^{10}$, $^{11a}$, $^{13a}$ fir = LXX κύριος |
| 16$^{11b}$ | κύριος | κύριος ὁ θεός | fir | | | |
| †18$^{1}$ | ὁ θεός | ὁ θεός | fir | Entire LXX (exc. c) | | |
| 18$^{13}$ | κύριος | κύριος ὁ θεός | fir | ej d$_2$ | | |
| †18$^{14}$ | τῷ θεῷ | τῷ θεῷ | fir | Entire LXX (exc. bw) | | |
| 18$^{17}$ | ὁ κύριος | ὁ θεός | fir | | | |
| 18$^{19a}$ | κυρίου | κυρίου τοῦ θεοῦ | fir | dgpt, etc. | | |
| *18$^{19a}$ | — | κύριος | i(uid)r | | | |
| 18$^{20}$ | κύριος | κύριος ὁ θεός | fir | ej$^{mg}$ | | |
| †18$^{22}$ | κυρίου | κυρίου | fir | LXX (except ej s$^{mg}$ v$^{mg}$ c$_2^{mg}$ Sah. Pal.) | MT | |
| 18$^{26}$ | κύριος | κύριος ὁ θεός | fir | ej s | | |
| 18$^{27}$ | τὸν κύριον | τὸν θεόν | fir | | | MT has אדני |
| †18$^{33}$ | κύριος | ὁ κύριος | fir | e pt bw | | |
| 19$^{13a}$ | κυρίου | κυρίου τοῦ θεοῦ | fir | l s$^{mg}$ | | |

## TABLE IV—*continued*

| | Camb. LXX. | fir. | MSS. | Agreeing with— | | Remarks. |
|---|---|---|---|---|---|---|
| | | | | LXX Authorities. | Other Texts. | |
| 19[13b] | κυρίου | κυρίου τοῦ θεοῦ | fir | dgp | | |
| †19[14] | κύριος | κύριος | fir | Entire LXX (exc. Boh.) | MT | |
| †19[16a] | κύριον | κύριον | fir | LXX (except ej OL) | MT | |
| 19[24a] | κύριος | κύριος ὁ θεός | fr | E | | |
| †19[27] | κυρίου | κυρίου | fir | Entire LXX | MT | |
| 19[29a] | κύριον | τὸν θεόν | fir | bw acm $c_2$ Pal. | | |
| †19[29b] | ὁ θεός | ὁ θεός | fir | Entire LXX (exc. ej) | MT | |
| †19[29c] | — | + ὁ θεός | f | | | |
| 19[29d] | κύριον | — | f | bw | MT | |
| 20[8] | — | + τον κύριον | fir | ej | | |
| 20[13] | ὁ θεός | κύριος ὁ θεός | fi | | | |
| 21[1a] | κύριος | κύριος ὁ θεός | fr | ej $d_2$ | | |
| †21[1b] | κύριος | κύριος | fr | Entire LXX (exc. aj e $d_2$) | MT | |
| 21[2] | κύριος | κύριος ὁ θεός | fr | dpt$d_2$ egj a s | | i *vacat* |
| 21[4] | ὁ θεός | κύριος ὁ θεός | fr | $d_2$ ej ac $ps^a$ | | |
| †21[6] | κύριος | κύριος | fr | LXX (exc. e $d_2$ Arm. Boh.) | | |
| 21[17b] | θεοῦ | κυρίου | fr | bw cm q | | |
| †22[9] | — | + τῷ κυρίῳ | f | b Sah. Eth. | | |
| †22[11] | κυρίου | κυρίου | fir | Entire LXX (exc. mj) | MT | |
| 24[1] | κύριος | κύριος ὁ θεός | ir | | | |
| 24[31] | κύριος | κύριος ὁ θεός | fir | | | |
| 24[40] | κύριος ὁ θεός | + μου | tir | acx Arm. | | |
| 24[48b] | κύριον | — | fir | h Eth. | | |
| 25[22] | κυρίου | κυρίου τοῦ θεοῦ | fir | Eth. | | |
| 25[23] | κύριος | κύριος ὁ θεός | fir | Eth. | | |

| | | | | | | |
|---|---|---|---|---|---|---|
| 26[2] | κύριος | κύριος ὁ θεός | fir | Eth. | | |
| 26[12] | κύριος | κύριος ὁ θεός | fir | Eth. | | |
| 26[22] | κύριος | κύριος ὁ θεός | fi[a]r | | | |
| 26[24a] | κύριος | ὁ θεός | fir | | | |
| 26[25a] | — | κύριος | fir | E dpt lsc Eth. | | |
| 26[25b] | κύριος | κύριος ὁ θεός | fir | Eth. | | |
| †26[28] | κύριος | κύριος | fir | Entire LXX (exc. cm Eth. Boh.) | MT | |
| 26[29] | κύριος | κύριος ὁ θεός | fir | s[mg] Eth. | | |
| 27[7] | κυρίου | κυρίου | fi[a]r | | MT | |
| †27[27] | κύριος | κύριος | fir | Entire LXX (exc. OL) | MT | |
| 28[4] | ὁ θεός | κύριος ὁ θεός | f | E | | |
| †28[13a] | ὁ κύριος | ὁ κύριος | fir | Entire LXX | MT | |
| 28[13b] | — | + κύριος | i[b]r | ackmost wxc$_2$ Arm. Sah. Eth., etc. | MT | |
| †28[20a] | — | + τῷ κυρίῳ | f | dp | | |
| †29[31] | κύριος ὁ θεός | κύριος ὁ θεός | fir | LXX (exc. bw acmo xc$_2$ Arm. Eth. OL) | | |
| 29[32] | κύριος | κύριος ὁ θεός | i[a]r | l | | |
| 29[33] | κύριος | κύριος ὁ θεός | fir | | | |
| 29[35] | κυρίῳ | τῷ κυρίῳ | fi | *D* dnp m | MT | |
| 30[30] | κύριος | ὁ θεός | ir | E Boh. Sah. | | |
| 31[3] | κύριος | κύριος ὁ θεός | fir | k Arm. Sah. Eth. | | |
| 32[9] | κύριε | + ὁ θεός μου | i[a]r | g | | f καὶ κς μου |
| *35[14] | — | + ὁ θεός | fi[a]r | bdnoptc$_2$ Boh. | | |
| †38[7a] | κύριος | κύριος | fir | Entire LXX ! | | |
| *38[10b] | — | + ὁ θεός | f | OL | | |
| 39[2, 3a, b, 5a, b, 21, 23a] | κύριος (7 times) | κύριος | fir | Entire LXX (practically) | MT | |
| *39[9] | τοῦ θεοῦ | τοῦ θεοῦ | fir | ditto | MT | |
| 39[23b] | κύριος | κύριος ὁ θεός | fi | | | |
| †49[18] | κυρίου | κυρίου | fir | ditto | MT | |

# TABLE V

## VARIANTS FROM MT FOUND IN THE SAMARITAN PENTATEUCH AND THE YOUNGER VERSIONS (PESHITTA, VULGATE, AQUILA, ETC.)

| | MT. | Variant. | Versions. | Agreeing with— | |
|---|---|---|---|---|---|
| | | | | Hebrew MSS. | LXX. |
| $3^{1x}$ | — | יהוה | Pesh.? | | |
| $3^{13}$ | יהוה אלהים | יהוה | Pesh.? | | *HP* arabs 4 |
| $3^{24}$ | — | יהוה אלהים | Pesh. | | fi$^{a}$ Chr. |
| $4^{1}$ | יהוה | אלהים | ὁ Ἐβρ., ὁ Συρ. Vulg.? | | LXX |
| $4^{10}$ | — | יהוה | Pesh. | | |
| $6^{3}$ | יהוה | אלהים | Vulg. | | *HP* 14, 73, 130(t) |
| $6^{5}$ | יהוה | אלהים | Vulg. | K80 | *HP* 76 |
| $7^{1}$ | יהוה | אלהים | Sam.? Pe. | K601, 686 | cw Arm.-codd. |
| $7^{9}$ | אלהים | יהוה | Sam. Vulg.? | K155 | E |
| $13^{10a}$ | יהוה | אלהים | Pesh.? | | LXX |
| $13^{10b}$ | יהוה | אלהים | Pesh. | | LXX |
| $14^{22}$ | יהוה | האלהים | Sam. | | |
| | | Om. | Pesh. | | LXX |
| $15^{6}$ | יהוה | אלהים | Pesh. Vulg. | | LXX |
| $20^{18}$ | יהוה | אלהים | Sam. | | bw ej Boh. Phil.-Arm. |
| $22^{11}$ | יהוה | אלהים | Pesh. | K248, 601 | |
| $22^{15}$ | יהוה | אלהים | Pesh. | | |

| | | | | | |
|---|---|---|---|---|---|
| 28[4] | אלהים | יהוה | Sam. | | Georg. |
| 29[32] | יהוה | אלהים | Pesh. | | LXX, Aq. Sym. |
| 30[24] | יהוה | אלהים | Pesh. | | LXX |
| 30[27] | יהוה | אלהים | Pesh. ? | | |
| 31[7] | אלהים | יהוה | Sam. | K69 | |
| 31[9] | אלהים | יהוה | Sam. | | |
| 31[16a] | אלהים | יהוה | Sam. | | |
| 31[16b] | אלהים | יהוה | Pesh. | | |
| 35[9b] | — | אלהים | Sam. | | LXX |

## TABLE VI

### PASSAGES IN WHICH HEBREW MSS. HAVE A DIFFERENT DIVINE NAME FROM MT

| | MT. | VARIANT. | MSS | AGREEING WITH. |
|---|---|---|---|---|
| $1^{2}$ | אלהים | Om. | K665 | |
| $1^{26a}$ | אלהים | Om. להם אלהים | K665, 650B * | |
| | | Rd. לאמר for ויאמר להם א׳ | K125 | LXX |
| $1^{28b}$ | אלהים | Om. | K2, *109*, *150*, 650M * | |
| $2^{18}$ | יהוה אלהים | יהוה | K*191* | |
| | | אלהים | K89 | $ec_2$ Eus. |
| $2^{21}$ | יהוה אלהים | יהוה | K*69*, *252* | h |
| $2^{22}$ | יהוה אלהים | יהוה | K89 | y Or.-Gr.; *al.* |
| $3^{1b}$ | אלהים | יהוה אלהים | K132 | E ej $v^{mg}$ Luc. |
| $3^{22}$ | יהוה אלהים | אלהים | K152 | $M^{txt}$ rell. Pal. Phil. Just. Chr. |
| $3^{23}$ | יהוה אלהים | יהוה | K*80* | b |
| $5^{22}$ | האלהים | Om. verse | K151 | [E *HP* 73, 74, 134] ? |
| | | Om. את האלהים | K*191* | [*HP* 79] ? † |
| $6^{5}$ | יהוה | אלהים | K*80* | Vulg. *HP* 76 |
| [$6^{13}$ | אלהים | יה אלהים ? | K152] ‡ | $D^{sil}$ ej *al.* |
| $7^{1}$ | יהוה | אלהים | K*601*, *686* | Sam. ? cw Arm. codd.) |
| $7^{9}$ | אלהים | יהוה | K*155* | Sam. Vulg. E. |
| [$7^{16a}$ | אלהים | יהוה אלהים | K95] § | |
| $^{15}$ | אלהים | יהוה | R*266 primo* | *HP* 19 |
| $15^{2}$ | אדני יהוה | יהוה אלהים | K95, *150*, 152 | |
| | | אלהים | K*80* | |
| | | יהוה אדני | K*384* | |

| | | | | |
|---|---|---|---|---|
| $15^{8}$ | אדני יהוה | יהוה אלהים | K*80*, *150*, *384* | Sah. Phil.-codd. |
| | | יהוה | K6, 178 | f |
| | | יהוה אדני | K9, *69* | |
| $15^{9}$ | — | יהוה | K*69*, *109* | |
| $16^{11}$ | יהוה | אלהים | R669 *pr.* | bw OL |
| | | אל | R*754 pr. m.*] ‖ | |
| $17^{15}$ | אלהים | יהוה | K189 | OL |
| $18^{3}$ | אדני | יהוה | K*155*, 178, 189, 244, 387 | |
| | | יהוה אדני | K9, *193* | |
| $18^{27}$ | אדני | יהוה | K*69*, 89, 111, 132 | |
| | | | R15, *16*, *197*, *251*, 293, *296*, *412*, 419, *464*, 611 *mg*, 688, *766*, 18 *pr.*, *592* | |
| | | יהוה אדני | K136, 244, *686* ¶ | |
| | | | R6, 245, 467 | |
| $18^{31}$ | אדני | יהוה | K11, 89, *227*, *239* | |
| | | | R18, *197*, *251*, *592*, *766* | |
| | | יהוה אדני | K*109*, *686* | |
| $18^{33}$ | יהוה | Om. | K*80* | c |
| $19^{18}$ | אדני | יהוה אדני | K108 | |
| $19^{29a}$ | אלהים | יהוה | K*199* | LXX |
| $20^{4}$ | אדני | יהוה | K9, 81, 132, *150*, 152, *199*, *227*, *239*, *601* | |
| | | | R419, 455, *507*, *766*, *248 pr.* | |
| | | Om. | K111 | |

* Not a MS. at all (see p. 82). † The LXX evidence is here distorted by Dahse (see p. 104).
‡ A clerical error wrongly cited as a variant (see p. 76).
§ Another clerical error. The MS. substitutes יהוה for the אתו preceding אלהים.
‖ אל corrected in margin *prima manu* to יהוה, therefore a *lapsus calami* (see p. 106).
¶ Kenn., but not de R., adds here K109, which has יהו אדני—another mistake of the same kind as $6^{13}$.

TABLE VI—*continued*

| | MT. | VARIANT. | MSS. | AGREEING WITH. |
|---|---|---|---|---|
| $20^{11}$ | אלהים | יהוה | K18, 125, 159 | |
| | | יהוה אלהים | K*109* | |
| | | Om. | K104 | |
| $22^{11}$ | יהוה | אלהים | K*248*, *601* | Pesh. |
| $28^{13b}$ | יהוה | Om. | K103 | LXX except ackmoxc$_2$ i$^{br}$ s t w Arm. Sah. Eth. and fathers |
| [$28^{20b}$ | אלהים | יהוה | K*193*]* | E egj Eth. |
| $30^{22b}$ | אלהים | Om. | K*170*, *185* | en Chr. |
| $30^{23}$ | אלהים | יהוה | K*69*, *80*, *157* | |
| $31^{9}$ | אלהים | יהוה | K*69* † | Sam. |
| $33^{11}$ | אלהים | יהוה | K189 | |
| $35^{9a}$ | אלהים | Om. | K13 | b |
| $35^{10}$ | אלהים | Om. | K13 | D dfhtc$_2$ Sah. Eth. Chr. |
| | | אלהים יהוה (!) | K5 | |
| $45^{5}$ | אלהים | יהוה | K128, *155* | Georg. |
| $45^{7}$ | אלהים | יהוה | K*4*, 128 | |

* A doubtful variant. Kenn. gives the reading as יש יהיה instead of יהיה. The slip is as likely to be in the יש as in the יהיה (for יהוה).

† Dahse adds "ras 229." Kenn.'s note is simply "ויצל אלהים [MT] sup ras 229." What right that gives to conclude that יהוה was *under* the erasure I cannot think. Whatever the mistake was, it must have been one affecting the *verb* as well as the divine name which follows.

# ADDITIONAL NOTES

## NOTE I

### KITTEL ON THE DIVINE NAMES

DAHSE opens his reply with a series of quotations from prominent scholars in "the most widely different universities of Germany," partly to show the profound impression which his own researches have made, and partly to introduce an expression of his amazement at the reactionary views to which I have given utterance (p. 481 f.). I have no wish to rob Dahse of any legitimate satisfaction he may find in the "admissions" or "concessions" of competent authorities, or their acknowledgments of the value of his work. But to prevent the general reader from being grievously misled, I think it right to say that he greatly exaggerates the extent of these admissions by partial quotation; and that the majority of the writers referred to —Wellhausen, Kittel, Sellin and Gressmann—still adhere to the documentary theory, and even in their estimate of the divine names are more nearly in agreement with my position than with his. I will show this in one instance—that of Kittel, to whose utterances Dahse several times appeals (pp. 481, 484, 485, 489, 495). From Kittel's *Geschichte des Volkes Israel*, Ed. 2, Bd. I, p. 255, Dahse (p. 481) quotes the following sentence: "Dahse is quite right in complaining that too little attention has been paid, on the part of the commentators and documentary school, to the state of the text." The best answer to that will be to translate the remainder of Kittel's important Note. I merely take the liberty of italicizing the points which support the views

I have maintained. After the sentence just cited, Kittel proceeds:

"Yet I can accept his conclusions *only to a very limited extent;* for one thing, because they rest on too narrow a foundation. It would require far closer attention to be bestowed on the books outside the Pentateuch—especially Chronicles, Isaiah, etc.—in order to arrive at secure results. I will summarize my examination in the following propositions: (1) In general, apart from Genesis i.–x., Yahwistic and Elohistic passages are easily recognizable even in the LXX (in spite of many variations between LXX and MT), *cf.* Genesis i. xvii. (also v. 1–28) with Genesis xii., xviii., xxiv.—(2) In Genesis ii. Elohim seems originally to have been wholly or in part the prevalent name; so also in Genesis iv., vi. 1–4, xv. 6 f. [I take it that here, as in (1) and (3), Kittel refers to the original LXX, but I admit that the meaning is not clear: see further p. 294 below.]—(3) In considerable sections of the Pentateuch we can perceive in the LXX—or it may be (*beziehungsweise*), in the Hebrew original of the LXX—the same tendency which we observe in Chronicles, *to substitute Elohim for Yahwe* (Exodus xvi. and xix.; Numbers xxii.). Along with that, to be sure, the tendency manifests itself *to assimilate the divine names to the prevalent* (Yahwistic or Elohistic) *usage of a section.*—(4) Yahwe-Elohim is occasionally, from the same tendency which MT exhibits in Genesis ii., iii., inserted in the LXX, or (*beziehungsweise*) in the Hebrew original of the LXX, in longer sections of Genesis ii.–x. From (1)–(4) it follows that in Genesis ii.–x., as also in isolated portions of the Law (cf. No. 3), the divine appellations of the MT are undoubtedly little decisive for the separation of sources. On the other hand, Dahse's thesis in its absolute generality *rests upon serious exaggeration.* For (5) it appears that within the LXX itself, particularly in Genesis ii.–x., there is great fluctuation in regard to the divine names (cf. $LXX^{E}$ in Genesis iii. 1, iv. 9, 13, vi. 22, vii. 9, 16, viii. 21, ix. 8, 26, x. 9, xi. 8: further $LXX^{D}$ in Genesis vi. 13, vii. 16, ix. 12, 17, xi. 9)—obviously not as a

result of transcriptional errors, but of such a kind as to show that the accommodation (? *Angleichung*) had been differently carried out in different Hebrew originals, *or was carried out differently by Greek copyists*. Further (6), the retention of Yahwe alongside of Elohim is, in particular, often wrongly explained by Dahse. That here *we have merely to do with a certain lack of consistency* appears from Chronicles. Finally (7) an immense number of cases of θεός in Isaiah for Hebrew Yahwe shows how strong was the tendency in certain later circles to avoid the Tetragrammaton."

Now, I do not subscribe to all these statements of Kittel—in particular I demur to his too easy (though still tentative) assumption of various Hebrew originals. But balancing one thing with another, I think that on the whole the passage yields as much general support to my contentions as I could reasonably expect from an independent quarter.

## NOTE II—Page 15

To break the force of the argument from Genesis iv. 26, Dahse has recourse to what is known as the Kenite theory of the origin of the name Yahwe. On p. 509 f. of his Reply he writes: "I conclude from certain phenomena that *Genesis iv. 26b relates to Cain*, and not to iv. 26*a*, Enosh. Cain, the representative of the Kenites, begins the invocation of Yahwe, but that is in entire harmony with the statement, Exodus iii., according to which Moses learnt to know Yahwe when in the service of Jethro the Kenite. And so Genesis iv. 26*b* ceases to be a *point d'appui* of the *documentary* theory, but becomes one for the *Kenite* hypothesis." This is an excellent example of exegesis divorced from historical and religious insight. The Kenite theory is all very well in its own place; and no doubt its advocates will be duly grateful to Dahse for this surprising confirmation of it. But he need not tell us that it was the theory held by Ezra or any other biblical writer. Does he expect a tolerably sane British public to believe that the Almighty,

in proclaiming himself the God of Abraham, Isaac and Jacob, should declare that he had concealed his true Name from these patriarchs, after having revealed it to the seed of Cain? And by what right does Dahse conclude from "certain phenomena" (which, by the way, are remarkably inconclusive) that iv. 26*b* has nothing to do with its actual context, but refers to Cain, whose story is finished nine verses back? And what of the exactly similar passages xii. 8, xiii. 4, xxi. 33, xxvi. 25, where the patriarchs are said to have *continued* the invocation of Yahwe begun by Enosh?

## NOTE III—Page 32

### EXODUS VI. 2, 3

Dahse's answer to the arguments of this chapter is disappointing in the extreme. Straitened as he was for space, I think he might well have spared a few of the pages he has spent in trying to convict me of inconsistency and ignorance for the discussion of this important and crucial theme. All that we get is the admission (Reply, p. 508) that my characterization of his reading is substantially just, along with an attempt to ward off the stroke of my criticism by interposing the impenetrable shield of the Pericope-hypothesis. "If this is treated as an independent narrative *it is certainly meaningless*; but regarded as a recapitulation it is in place." Exodus vi. 2 ff. is the "chapter heading" to the 50th Seder of the Pentateuch, and "contains a recapitulation of what is narrated in Exodus iii." (which stands, by the way, in the last Seder but one). It belongs to the character of these chapters that they "originally contained nothing new"; therefore, I suppose, they need not be expected to make sense. To this I might reply, in the first place, that one does not readily apprehend how a sentence in itself meaningless becomes meaningful when regarded as the reproduction of another writer's meaning, especially when that writer's words had been read in the synagogue two weeks before this commentator is allowed to be heard.

Secondly, that its intrinsic meaninglessness is entirely due to the arbitrary excision of the two names Yahwe and El Shaddai and forms therefore a very strong argument against such excision. Thirdly, that Exodus vi. 2 ff. is neither a recapitulation nor a repetition of Exodus iii. It may refer to the same epoch-making event—the first revelation of the name Yahwe—but the whole conception of that event—of the manner in which the revelation came, of the time, place and circumstances in which it was given—as well as the terms in which it is described, are totally different. The writer who undertook such a radical transformation of another narrative could not afford to write meaningless sentences. I might go on to urge that I have shown in general (Chapter VII) that the theory of chapter headings does not come well out of a detailed examination —that the passages classed by Dahse under that head have only in the rarest instances the character of headings or even of *mere* recapitulation, that they contain much that is new, and that altogether they present the unmistakable aspect of a thoroughly independent presentation of the national traditions of Israel. But I am afraid it is of no use. The obsession of the Pericope-hypothesis has a neck like an iron sinew, and the logical weapon is not forged that will cut it. There is nothing for it but to imitate my opponent's obduracy of heart, and repeat what I have said before, that there is no part of the *Textkritische Materialien* that has failure and futility more legibly written on the face of it than the discussion of Exodus vi. 2, 3.

## NOTE IV—Pages 84, 251

A slight inspection of Tables III and IV suffices to show how incomplete and undiscriminating Dahse's published treatment of the recensions **egj** and **fir** is. It is obvious even to the uninitiated in LXX criticism that the great difficulty in such investigations is to determine how far a given recension is based on the prevailing text of the LXX, and how far the recension has imparted its peculiar

complexion to that prevailing text. Two methods may be attempted: we may either take account of *all* the readings of the recension under examination and try to form a rough idea of its general tendencies; or we may consider only those readings in which it stands alone, or is supported by but a few other MSS. The second of these methods alone can lead to decisive results. But then what are we to make of those passages where the recension has the same name as the ordinary LXX? With regard to these we are exposed to all the disadvantage of the more sweeping method of study: we cannot tell whether these readings belong to the recension, or whether MSS. of the recension have been accommodated to the vulgar LXX text. At all events, so far as I can judge, Dahse adheres neither to the one method nor to the other. In dealing with egj he follows on the whole the stricter plan, and cites mostly cases in which that recension departs somewhat markedly from the general LXX tradition. But even there it would seem that he should have excluded the majority of those readings which I have marked with †, and have included many of those marked.* In fir, on the other hand, he takes in a much larger number of common readings; and there again he has omitted several which appear highly characteristic of the recension. The result is that in the one case his survey covers rather more than one-eighth, in the other rather less than one-third, of the names of the LXX in Genesis. It seems to me that both in quality and in quantity such data as are given form a very precarious basis for ascertaining the characteristics of a particular recension. If they point to any conclusion at all, it is that the authors of these recensions followed no consistent principle in their treatment of the divine names, but yielded to different *tendencies* in different sections of the book of Genesis. It also appears, I think, that egj possesses far more the character of a κύριος ὁ θεός-edition of Genesis than fir: in the first case two-thirds, in the second only about two-fifths, of the passages cited have the double name.

## NOTE V—Page 85

With reference to this note, Dahse claims (Reply, p. 497) that I *too* admit that *ὁ κύριος* is equivalent to אדני. There he mistakes my meaning, if not his own also. I have allowed, on the positive evidence of two occurrences and the negative evidence of a certain number of vocatives, that an אדני in the Hebrew may plausibly be said to be represented by *ὁ κύριος* in Greek, whenever the article is admissible; but I have not admitted that every *ὁ κύριος* in Greek stands for אדני in the Hebrew. Nor do I think that this can have been Dahse's own view when he wrote as follows in the *Textkrit. Mater.*, p. 38 f.: "Es wird also, wenn in der LXX an diesen Stellen *das ursprüngliche* יהוה mit *ὁ κύριος* (nicht wie später gewöhnlich *κύριος*) stehen geblieben und nicht in *κύριος ὁ θεός* umgewandelt ist, Wert darauf gelegt, dass man in diesen kultischen Dingen *nicht an irgendwelche Elohim, sondern an Jahweh* sich gewandt habe." Perhaps he now sees the force of my contention that in such cases, whatever be the difference between *κύριος* and *ὁ κύριος*, the presence or absence of the article is a peculiarity of the Greek translation which can answer to nothing in Hebrew.—On p. 488 of the Reply he objects to my description of the sentence above quoted as an echo of Eerdmans' polytheistic theory, saying that he had put forward this view as long ago as 1903, and that he "never knew before that an echo could be heard as much as five years before the actual sound!" Of course, when Dahse assures me that his position in 1912 is the same as in 1903 I accept the correction and the rebuke. But I may be permitted to remark that the nearest approach to it which I could have found in the *ARW* for 1903 is in these words from p. 312: "Dann wäre Jahwe nur stehen geblieben an den beiden Stellen, *wo von einem Opfer die Rede ist* $4^{3}$ und $8^{20}$, und in der sprichwörtlichen Redensart $10^{26b}$." If the reader will compare the expressions italicized in these two extracts, he will understand how the echo of Eerdmans'

which I seemed to detect in the more recent was not audible in the earlier.

## NOTE VI—Pages 93, 237

### THE ARCHETYPAL MANUSCRIPT

This brief and summary description of the archetype is quoted by Dahse on p. 499 of his Reply, and followed up by an interrogation which I should call quite pertinent and legitimate if I did not know that he would seize on these expressions, and say that I had yielded the point at issue: "Granted, now, that the way in which this text has been preserved shows that it was almost superstitiously revered, do not those very peculiarities prove that even before the Roman period this text was not preserved in its purity either?" The correct answer to that question is that while as a matter of fact the text has not been preserved in perfect purity, yet the fact is not necessarily proved by the peculiarities here referred to. Since my statement, however, is couched in terms which may suggest wrong ideas to a reader unfamiliar with the subject, I will here draw out its implications somewhat more clearly.

1. The description refers to the MT as a whole, and the eccentric features spoken of are much less observable in the Pentateuch (and especially in Genesis) than in other parts of the Old Testament. It is well known that the Pentateuch is the part of the Hebrew Bible whose text has been best preserved; and the phenomena on which the present argument depends are there comparatively unimportant. In Genesis there are cases where the traditional reading (*Qerî*) departs from the consonantal text (*Kethîb*), there are a few dotted words or letters, a couple of gaps in the middle of a verse, and a few *literae majusculae* or *minusculae*. The more serious cases, of inadvertent dittography, or of omission of a word necessary to the sense, do not occur there, or in the Pentateuch anywhere. Thus while there were slips in the archetype, there is *so far* no evidence that there were serious slips, or any that had gone unnoticed and uncorrected.

2. But the more important point is that the description refers to *accidents*, and not to the *substance*, of the text. The distinction can be made plain by a very homely illustration. An author sends to the printer (such cases have been known) a MS. full of corrections and other marks of haste. Words are scored out and rewritten above, omissions are marked by a caret and supplied in the margin; there may be a blot of ink on this page and an illegible smudge on that. Yet in substance that may be a perfect text—may, in fact, be the original autograph. But if the printer proceeds to reproduce all these accidental peculiarities of his copy, then we know that he has been guided by some other motive than a desire to express his author's meaning. Now, so far as the gist of my argument is concerned, that is exactly the case of the MT, except that the mistakes and corrections were not made under the eye of the original writer but by subsequent copyists, and that the reproductions are not due to a single printer, but to thousand of scribes working separately. The phenomena in question prove a superstitious regard for the eccentricities of some one MS., but they do not of themselves prove (at least as regards the Pentateuch) that the text of that MS. was in substance corrupt.

Hence it can be rightly maintained that, in spite of this strange episode in its history, the Massoretic recension has preserved the ancient text with relative fidelity. That in places it is corrupt we know from other considerations, chiefly internal. Some of these can be corrected by help of the versions, some by conjecture; others are irremediable. But taking it as a whole, and in comparison with other authorities, what I have elsewhere said remains true, that "the Hebrew text possesses credentials to which no version . . . can pretend."

## NOTE VII—Page 108

On Gen. xvi. 11 Dahse calls attention to the fact that Procksch in his recent commentary on Genesis takes sub-

stantially the same view as Wiener and himself, although he says, without sufficient warrant, that the reading behind the ὁ θεός of bw and OL was *El* and not *Elohim*. Similarly on xxxii. 31. I was not aware of this fact when I wrote, but had noted it since. Of course I regret that so excellent a scholar should countenance an opinion which seems to me so indefensible; but I think it is due to his not recognizing the distinction between the original popular etymology and the form into which that etymology would naturally be cast by a writer habitually using Yahwe as the name of the true God. If Procksch had said that this was a case in which the Yahwist *might* use Elohim without being untrue to his principle, I should have heartily agreed; but when he says (or implies) *must*, I entirely demur, for the reasons I have already given.—As to the analogy of 1 Sam. i. 20, I do not see what Dahse supposes he gains in argument by appealing to the opinion of Budde that the LXX with its σαβαώθ has preserved the original reading. If the original name was *Yahwe Zebaóth*, my argument is rather strengthened than weakened; for if the solitary *Yahwe* is not equivalent to *El*, *Yahwe Zebaóth* is so still less; and if the latter could nevertheless be substituted for El in the explanation of an etymology, so *a fortiori* could the former.—Dahse adds "by the way," and somewhat naïvely, that he does not know any passage in Wiener's writings in which he expresses the opinion "that Ishmael is a fictitious name"! I never supposed that Wiener would be so far left to himself. But when he says that under given circumstances Ishmael *must* have read Ishmayah, in what other sense can his words be understood?

## NOTE VIII—Page 127

### WIENER ON THE RELATIONS BETWEEN THE SAMARITAN AND THE SEPTUAGINT

The freer scope afforded by this volume enables me to enter a little more fully into the details of Wiener's argument on this subject. I have said that the great bulk of

the differences registered by him are due to the freedom of translators who did not render word for word, etc. The mistakes which vitiate his criticism are (1) that he makes too little allowance for the licence natural to the work of translation, and freely employed by the LXX, such as non-literal rendering, substitution of synonyms, accommodation to Greek idiom, adjustment of forms and expressions to the context, explanatory additions, and so on, but tends in all cases to assume a divergent Hebrew; and (2) that he has not recognized the necessity of showing that this Hebrew is intrinsically superior to that of MT and Sam., before his main argument can be sustained. It seems to me that his critical method could be at once illustrated and reduced to absurdity by applying it to the English Bible. It is known historically that our authorized version was based on the MT. But if that fact were unknown, a critic setting himself on Wiener's principles to reconstruct its hypothetical Hebrew basis might easily convince himself that it was made from a text considerably different from the Hebrew *receptus*. Suppose we try our hand on Isaiah vi.—by no means a *corpus vile* for such an experiment.

*v.* 1. בשנת מות: *In the year of the death of;* A.V. "In the year that . . . died" = בשנת מות.

ואראה, *and I saw;* A.V. "I saw also" = ראיתי גם.

ושוליו, *and his skirts;* A.V. "and his train" = ושולו.

מלאים, *were filling;* A.V. "filled" = מלאו.

*v.* 2. שרפים עמדים, *seraphim were standing;* A.V. "stood the seraphim" = עמדו השרפים.

שש כנפים: a clear case of dittography! A.V. rightly, "six wings."

בשתים, *with two;* A.V. "twain": Heb. uncertain.

יעופף, *he flew;* A.V. "did fly" = עוף יעופף (Inf. Abs.).

*v.* 3. זה אל זה, *this to this;* A.V. "one to another" = איש אל רעהו.

מלא, *the filling;* A.V. "is full" = מלאה (an emendation already suggested: see Kittel, *BH*).

*v.* 4. הספים, *the thresholds;* A.V. "the door" = הדלת.

*v.* 5. *and in the midst . . . I dwell;* A.V. reverses the order.

*v.* 6. במלקחים לקח, *with tongs he had taken;* A.V. inserts אשר and the art.

*v.* 7. ויגע, *made to touch*; A.V. "laid" = וישם.

And so one might go on finding traces of a variant Hebrew in every clause, where we know that there are only loosenesses of rendering, a regard for idiom and rhythm in English, or slight misunderstanding of a word or a construction.

Now it is quite true that Wiener allows in words for the occurrence of such divergences between the Hebrew and the LXX; but in practice he does not carry the admission nearly far enough, and I do not think that what I have said is a very seriously exaggerated caricature of his method. Let us look at some specimens; and I will for the most part confine myself to those instances where he has the strongest apparent case.

Genesis xiii. Here the following are claimed as clear cases in which the LXX found different consonants from the MT:

*v.* 3. למסעיו, ὅθεν ἦλθεν; What is the variant Hebrew here? Not אשר הלך משם surely? Is it ממסעיו? Or ממוצאיו? In any case, if there be a consonantal variant at all, it is certainly inferior to MT. In reality ὅθεν ἦλθεν is simply a loose conjectural rendering; the LXX are always at a loss when they come to the noun מַסָּע (see Exod. xvii. 1, xl. 36, 38; Num. x. 2, 6, 12, 28, xxxiii. 1, 2; Deut. x. 11).

מנגב, εἰς τὴν ἔρημον. It is just possible (though very improbable) that this represents a Heb. בנגב (מ and ב being frequently confounded); but if so it is certainly to be rejected. Abram starts *from* the Negeb (*v.* 1).

*v.* 4. בראשנה, τὴν σκηνήν. There can be no question as to the inferiority of LXX here. But I think we can trace the source of its error. σκηνήν is in fact the worst attested of all readings to this passage; and the best is ἀρχήν, which answers to MT. I venture to conjecture, on the basis of the various readings in the Cambridge LXX, that

the original LXX, slightly paraphrasing the Hebrew, ran thus: *εἰς τὸν τόπον οὗ ἐποίησεν ἐκεῖ τὸ θυσιαστήριον τὴν ἀρχήν* (cf. Dan. ix. 21 [LXX], viii. 1 [Theod]); *τὸ θυσιαστήριον* was then either accidentally dropped, or carelessly moved to correspond with the Hebrew, and the nonsensical text thus arising was afterwards amended in A ny by changing *ἀρχήν* to *σκηνήν*.

*v.* 9. הלא, *καὶ ἰδού*. Is Wiener prepared to say that this necessarily presupposes והנה wherever it occurs (e.g., Deuteronomy iii. 11, Joshua i. 9, Judges vi. 14 and often)? If so, what becomes of the idiomatic זה in Deuteronomy ii. 7, viii. 4? Is this also to be changed to הנה because of *ἰδού*? And would הנה be in any way preferable to הלא?

*v.* 9. Omission of נא. This is one of the commonest things in the LXX: cf., xiii. 8, 14; xxiv. 2, 12, 14, 17, 23, 42, 45; xxv. 30; xxvii. 3, 21, 26; xxxi. 12, etc. Are we really to suppose that in all these cases the LXX did not find the נא in their original? And that the omission (here or elsewhere) improves the text?

I will not discuss the four instances where the LXX has *ὁ θεός* for the Tetragrammaton of MT, because there can be no proof either that the translators found Elohim in their Hebrew exemplar, or that if they did it is a more original text than Yahwe.

Exodus xvii.

*v.* 1. לשתת העם, *τῷ λαῷ πιεῖν*. The Hebrew here is (*pace* Kittel!) perfectly idiomatic: see 2 Samuel xvi. 2, לאכול הנערים and לשתות היעף; Isaiah li. 10, לעבר גאולים.

*v.* 3. מקני, בני, אתי. If the "our" of LXX., Vulg., Pesh., Targ. Jon. for "my" of MT had been original, how does Wiener explain the change to sing. in MT? Evidently these versions have accommodated to the העליתנו of MT. In Deuteronomy xxxi. 16–21, e.g., LXX has plurals for sg. throughout, but so also have A.V. and R.V., exactly as LXX. Yet we know that the EVV read the MT!

*vv.* 5, 6. *τοῦ λαοῦ* for ישראל and *ὁ λαός μου* for העם are changes as likely to have been made in Greek as in Hebrew, and in neither is there a superiority on the side of the LXX.

On the other hand זקני (*v.* 6) is manifestly better than *τῶν υἱῶν*. There could hardly be a clearer case of careless rendering than this. The trite phrase "children of Israel" came to the translator's pen, and as it did not utterly spoil the sense, it was never afterwards altered. In *v.* 7, on the contrary, בני is in place.

*v.* 16. The omission of ויאמר by the LXX is, we are told, "doubtless the correct text." Why "doubtless"? If the word is not essential, the omission is certainly no improvement.

Leviticus xvii. Here we are told that "most" of some twenty-three variants registered point to a different consonantal Hebrew text, but that it is a *better* Hebrew Wiener does not venture to assert. It seems to me that the only cases where the suggestion of a divergent Hebrew can be seriously entertained are (*a*) *τῶν υἱῶν* for מבית in *vv.* 3, 8, 10: here the presumption is strongly in favour of MT, which keeps to מבית throughout, except in *v.* 13, where MT has מבני, but Sam. and 9 MSS. have the (probably original) מבית. There is no reason why the substitution should not have been made in Greek. (*b*) The addition in *v.* 3 of *ἢ τῶν προσηλύτων ἢ τῶν προσκειμένων ἐν ὑμῖν*. But this is supplied from *vv.* 8, 10, 13; and, from the MS. evidence, it is more than doubtful if it formed part of the original LXX. It is therefore probably to be regarded as an inner-Greek corruption. (*c*) In *v.* 8 *ἐν ὑμῖν* does not necessarily imply a Hebrew בתוככם, but even if it did, the בתוכם (3rd person) of MT is undoubtedly right. (*d*) So in *v.* 11 הבישר of MT is decidedly preferable to the כל בשר of LXX and one Hebrew MS.; and this explains away at the same time the twice added *αὐτοῦ* of LXX to *αἷμα* in the same verse. (*e*) The additions of *τὰ ἱμάτια* and of *ὕδατι* in *v.* 16 carry no marks of originality.

In Numbers xix. 1–7, Wiener only claims that certain words wanting in the LXX are probably glosses. There he gives expression to a fallacious assumption which appears often in his writings, that the shorter text is always to be preferred. The only case mentioned that

looks plausible at first sight is the second הכהן in *v.* 7. But, unfortunately for Wiener, that is *not* omitted by LXX (only by two cursives)!

On Deuteronomy xx. 1–12 it is unnecessary to dwell, since Wiener only detects there evidence of "differences of pronunciation of the same consonantal text." But there is one case of *mistaken translation* which he ought not to have passed over. In *v.* 6 ולא חללו and יחללנו are rendered respectively by *καὶ οὐκ εὐφράνθη ἐξ αὐτοῦ* and *εὐφρανθήσεται ἐξ αὐτοῦ*. That is to say, the LXX read חלל as הלל.

I am really in doubt, even after reading Dahse's spirited defence of Wiener, whether "transparent incompetence" and "hastily improvised scholarship" be after all expressions too harsh to describe textual criticism of this order. At all events I am justified in saying that he has contributed nothing of value in these investigations to clearing up the relations between the LXX and Sam. He has in no case *proved* that the LXX goes back to a Hebrew original superior to the Sam. and MT. He has often *assumed* a Hebrew basis which is worse, and sometimes impossible; and to that extent his argument goes to show that the ancestry of the LXX has undergone corruption since the time when it parted from that of Sam. and MT. At the same time, I draw a distinction between his detailed criticism of the text and the general principle on which his reasoning proceeds. If he would improve his methods, and exercise greater circumspection, I do not doubt that he will succeed in finding cases where the LXX represents a Hebrew superior to either MT or Sam., or both combined. His general theory is not *proved*, nor do I accept it, but I still admit that it "has a claim to consideration."

## NOTE IX—Pages 145, 154

### THE DIVINE NAMES IN THE VULGATE

Since writing these passages my attention has been drawn to an article by the Rev. Hugh Pope, O.P., which

appeared in the October number of the *Irish Theological Quarterly* (1913, pp. 375–398). The writer takes a very definite stand by the side of Wiener in the controversy about the critical value of the divine names, and even goes so far as to defend his most scurrilous diatribes as being "necessary"! I will not here deal with Mr. Pope's view of the general situation, nor will I help him to answer the question he has put at the head of his article: "Where are we in Pentateuchal Criticism?" I have expressed my mind on every aspect of the case on which he has touched; and he has contributed nothing which moves me to reopen any part of the discussion. The interest of the article lies solely in its attempt to use the Vulgate to destroy confidence in the accuracy of our present MT. I may congratulate myself on having anticipated this line of attack. A considerable part of the article is an elaboration of the hint which I dropped in the note on p. 154, where I have called attention to the variations in MSS. of the Vulgate as a quarter where Dahse and Wiener would find some more grist for their mill. Of course I am aware that Pope was in no way indebted to that hint for the inception of his argument; but I am none the less grateful to him for having brought out so clearly how very little is to be gained by following it up. That, to be sure, is not his opinion; but I will try to show in a few words that it is the true estimate of his results.

The first point to be considered is the relation as a whole of the Vulgate to the MT. It is the common judgment of scholars that the Hebrew basis of the Vulgate, while not absolutely identical with the present MT, very closely resembles it. I believe that what I have said on p. 144 f. expresses the truth. The proved deviations of the Hebrew basis of the Vulgate from the MT are for the most part well within the limits of probable scribal error subsequent to the fixing of the standard text. It is only where the Vulgate presupposes a Hebrew reading intrinsically superior to the MT, or one supported by an older

version, that we have any right to look behind the Massoretic recension, and suspect the survival of an earlier type of text. In all other cases we must go on the presumption that the divergence has come in through mistakes in copying the standard text; and of course in each case of difference it is a question whether the Vulgate or MT has preserved the original text as fixed by the school of Aqiba in the second century. Now the actual extent of this divergence is, I am convinced, greatly exaggerated by Mr. Pope. He refers his readers to Exod. xxx., xxxv. 17–xxxvi. 16, xxxvii. 7–19, xxxix. 8–21, xl. 9–23, as passages where "it will be seen at once that St. Jerome has a consistently shorter text" (p. 385). Well, I have read these passages; and have formed the opinion that even in these selected and highly technical and difficult sections the amount of probable divergence between the MT and the underlying Hebrew of the Vulgate is small. It would not be right to express a confident judgment without more careful study than I can afford to make of the subject; but my strong impression is that, while textual differences exist, the chief cause of variation between the Vulgate and the MT is condensed paraphrase in translation. And even if the textual difference should be greater than I take it to be, the passages cited are such as, from their technicality and redundancy, were peculiarly liable to errors of transcription. Mr. Pope will have to extend his investigations to a fairer field of comparison before he can claim to have proved his thesis.

A much more serious question is raised by Pope's attempt to prove that Jerome used widely different Hebrew MSS. at different periods of his life. The argument is to this effect: The Vulgate of Genesis (translated about 404 A.D.) gives us the text of a certain Hebrew MS. which closely corresponded with our MT. But in 388 or 389 (I accept the date from Mr. Pope) Jerome wrote a series of *Quaestiones in Genesim:* and here he uses a Hebrew text which differs widely from MT and from the Hebrew basis of the Vulgate. Now there is no use

mincing matters: this reasoning is intelligible to me only on one supposition, viz., that Mr. Pope has fallen into the gross blunder of fancying that in the *Quaestiones* Jerome is commenting on a Hebrew text. It is true that Jerome's practice is not perfectly uniform in this respect. On xlix. 22 ff., for example, he tells us that he translates direct from the Hebrew, "quia LXX interpretes in plerisque dissentiunt"; and in a very few other instances we find him tacitly doing the same thing, and whenever he does so it is our MT that he uses. But apart from these very rare and exceptional cases, it is as certain as anything can be that the *lemmata* on which he bases his exposition are not taken direct from the Hebrew, but (directly or indirectly) from the LXX; and the only doubtful question is whether he is citing the Old Latin version of the LXX or translating from the LXX itself. His own words are: "Et quo facilius emendatio cognoscatur, *ipsa primum ut apud nos sunt testimonia proponemus*, et ex collatione eorum quae sequuntur quid in illis, aut minus, aut plus, aut aliter sit, indicabimus." It has been too readily taken for granted by scholars that the reference here is to MSS. of the OL version; and indeed readings are constantly cited as OL which have no other authority than Jerome's *Quaestiones*. On the whole, however, the evidence points to the conclusion that the text annotated is not mere transcription of the Latin, but Jerome's independent rendering of the Greek. But, be that as it may, any one who reads a few consecutive pages of the *Quaestiones* will speedily be convinced that whatever Jerome is doing he is not translating from a Hebrew MS. His *references* to the Hebrew are frequent and detailed, and in no case (except בחם for בהם in xiv. 5) do they imply a consonantal text different from our MT. The whole argument, therefore, crumbles to pieces. It is human to err; but it is idle to pretend that an error of this magnitude leaves unimpaired our respect for Mr. Pope's competence to deal with the problem he has taken in hand.

Let us come now to the divine names in Genesis.

Mr. Pope, it appears, has had access to 18 MSS. of Genesis in the possession of the Papal Commission entrusted with the Revision of the Vulgate. Any one who has glanced through the pages of Vercellone will be prepared to hear that these contain numerous variants from the standard Clementine edition. But the results, as given in this article, are unexpectedly meagre. Pope gives a list of 16 readings in chap. i.–xi., where the Clementine Vulgate differs from the MT. To seven of these there are MS. variants; and with one exception (ii. 16: one MS.) the variant MSS. confirm MT. Of the 16 divergences, twelve are omissions of the name by the Vulgate (i. 4, 5, 17, 26, 28, ii. 3, 16, iii. 22, vi. 6, viii. 1, 21, xi. 9). In four cases (iv. 1, vi. 3, 5, vii. 9) the Vulgate reads a different name from MT. Let us dispose of these four first. They were all allowed for and duly recorded in my *Expositor* articles, except vi. 3, which, following Dahse, I had inadvertently omitted. But we learn further that in iv. 1 and vii. 9 there is strong MS. support for the name which agrees with MT, and that in both these cases the reading of MT is preferred by Cardinal Carafa, the editor of the Louvain Bible. The net result of the investigation, then, is that I have added vi. 3 to the Table, and put a mark of interrogation against iv. 1 and vii. 9, making the necessary alterations in the text.

As to the twelve omissions of the standard Vulgate, they were all known before; the MSS. as cited by Pope adding no new case. Since even Dahse does not record them, we may conclude that he considers them of no importance as textual evidence. There he is undoubtedly right. The omissions are not textual, but are incidents of the translation. Pope recognizes this as a possibility, but asks (p. 388) who is to say that the omissions did not occur in the Hebrew text that was being translated. No one with any sense of Hebrew idiom, or who has considered Jerome's practice as a translator, will have any hesitation in answering that question. And here I will make the general observation that I have long thought

that in much recent textual criticism there is a tendency to make too much of the minute differences of the Vulgate from the MT. These, in a large number of instances, are purely stylistic: such things, e.g., as substitution of the passive for the active, of a relative for a co-ordinate sentence, of a participial construction for the finite verb, and so on. And hardly anything is more characteristic of the style of the Vulgate than the omission of a proper name, when the sense is clear without it. If Mr. Pope will turn once more to Genesis xi. 9, he will see that the "Yahwe" could not possibly have been wanting in the Hebrew, and that its omission in the Vulgate is due entirely to the substitution of the passive for the active construction. And perhaps he will allow that Jerome's words which he quotes on p. 386: "Non debemus sic verbum de verbo exprimere ut dum syllabas sequimur perdamus intelligentiam," have a closer application to the question in hand than he has realized. It is of no avail to say that "he is only speaking of those turns of expression which the idioms of the language used demanded," when we see that Jerome was capable of adopting a turn of expression that carried with it the omission of a divine name.

Pope seeks to upset this explanation by instancing cases, chiefly from the Epistle to Sunnias and Fretela, where Jerome insists on the Hebrew text of a divine name as alone correct. But what is Jerome doing in the letter to Sunnias and Fretela? He is answering a set of specific questions on the text of the Psalter propounded by these two correspondents, who were troubled by the discrepancies between the Latin Bible and the LXX, and asked him which was most consonant with the Hebrew. Jerome was not the man to put off such inquirers by telling them that it did not greatly matter! He could read the Hebrew, and naturally he told them exactly how it stood. It is true that he occasionally insists on the importance of the Hebrew, as in Ps. lxxi. (lxxii.) 18, where he finds in the threefold divine name of the MT an allusion

to the mystery of the Holy Trinity. But it does not in the least follow that where he saw no such important meaning in the name he would have scrupled to omit it in translation, for the sake of a more elegant Latin sentence. I willingly grant that he would not wantonly change one divine name to another; but that he did not consider a divine name *per se* to be inviolable is shown by the fact that he did not think it necessary to correct the OL by the Hebrew in those passages of the *Quaestiones* where Mr. Pope supposed that he was quoting a divergent Hebrew text.

Mr. Pope's overestimate of the significance of the variant divine names of the Vulgate springs from the same lack of *circumspection* (in the literal sense of the word) which is so manifest in the work of Dahse and Wiener. He has concentrated his attention on a small set of phenomena, within a narrow field of vision, and appears to be totally oblivious of facts outside that field which have to be taken into account before we can justly appreciate the evidence of the Vulgate. There are indeed a great many circumstances which conspire to reduce to a minimum the probability that any reading of the Vulgate goes back to a Hebrew independent of the Massoretic recension. (1) It is a well-established fact that the standard text of the O.T. was fixed by Jewish authority about the middle of the second century. (2) It is equally certain that from that time onward a determined effort was made in Jewish circles to secure the universal ascendancy of that text; and the divine names are about the last element of the text with regard to which laxity would have been permitted. (3) We know from the younger Greek versions and from Origen that this type of Hebrew text was thoroughly established in the third century after Christ: the translation of the Vulgate was not commenced until 390 A.D. (4) Jerome is known to have put himself to great trouble and expense to procure the most authoritative Hebrew MSS. and the best Jewish instruction: it is incredible that in these circumstances he should have been dependent

on MSS. belonging to another than the standard recension. (5) The Vulgate itself shows that its Hebrew basis belongs to the same recension as our present MT, and seldom varies from it beyond the limits of transcriptional error. (6) Even the best text of the Vulgate does not accurately represent the work of Jerome. It is well known that the two versions—the Old Latin and the Vulgate—were current together, in a keen struggle for existence, in the Western Church till the seventh century (in some localities long after that); and it is the judgment of all authorities on the subject that the purity of each text has been contaminated by intrusions from the other. How far this goes to explain the slight divergences that exist in the divine names it is impossible to say; for no form of the OL is extant for any of the cases I have found except xv. 6, and there OL (but also the entire LXX) agrees with the Vulgate. (7) Jerome aimed even less than the LXX at a word-for-word rendering, or a style of translation that sacrificed Latin idiom to a slavish literality. When we consider all this, and observe in addition that after all there are only about *three* thoroughly attested variant divine names in the Vulgate of Genesis—the omissions being due to reasons of style—we shall not rate very highly the contribution which the criticism of the Vulgate is fitted to make to the controversy regarding the divine names in Genesis.

## NOTE X—Page 146

In this passage, and also on p. 6 ff., Dahse thinks (Reply, p. 505) that I seek to minimize the value of the divine names for the division of Genesis, in order to escape the consequences of the uncertainty of the text. No doubt! If I say that Dahse "exaggerates" the importance of the divine names, quite naturally, from his point of view, I seem to be "minimizing" their importance. The question, however, is one of fact: does Dahse assign to the names for God a higher importance in the analysis of Genesis than they actually possess in the critical process by which

that analysis has been carried out? The answer to that is not doubtful. On p. 7 I have quoted his words to the effect that even to-day the whole division of sources still depends on the use of the divine names; and I have gone on to show that that is absolutely untrue. I have further pointed out in a note on the same page that he tries to make good his assertion only by five pages of garbled quotations from Gunkel and others, from which he omits all the other criteria which are adduced along with the divine names—a procedure which I am glad to see that Sellin also has characterized as grossly unfair. And a similar unfairness appears in Dahse's use of a quotation from Gressmann on this same page (505). Gressmann is there speaking of the analysis in the middle books of the Pentateuch: he says that there is substantial agreement on the delimitation of P, but that the separation of J from E can seldom be effected with an approach to certainty, because the criterion of the divine names *which has approved itself in Genesis* fails almost entirely in the middle books of the Pentateuch: all which is exactly what I have myself said on p. 9! It is true that Gressmann goes on to say that the demonstrative force of linguistic usage is quite small in view of the poverty of the Hebrew; but Dahse has no right to assume that that remark applies to Genesis, for on the previous page of his book Gressmann has expressly spoken of the contrast between the masterly redactional work which we find in Genesis and the confusion which prevails from Exodus to Numbers. And when finally Gressmann speaks of J and E as in many cases nothing more than "labels" (in Exod.–Num.) which may be exchanged at will, his meaning is quite plain from a sentence a little lower down, which Dahse forbears to quote: "Moreover, the contradictions and doublets remain, even if one is in doubt whether a variant belongs to J or to E." In other words, there are many cases where we can be sure that two accounts are interwoven, although, in default of the criterion of the divine names, and because of redactional confusion, we cannot determine which is

J and which is E. It will be seen how far it is true that in my estimate of the utility of the divine names in Genesis I argue "quite oppositely to Dr. Gressmann."

## NOTE XI—Page 147

### THE UNCERTAINTY OF THE HEBREW TEXT

When I wrote this sentence, I thought that I was stating a somewhat formidable objection to Dahse's critical work. I expected him to repudiate my inference, and to claim that he had done something positive to establish the true text of the divine names. But it appears not. On the contrary I have "rejoiced" him by these words: "Such is, in fact, the case" (Reply, p. 505). Vague uncertainty, it would seem, is what he aims at. And again (p. 506) to Gressmann's assertion that the whole Pentateuch must be corrected (i.e. *altered*) word for word before the ground is cut from beneath the documentary hypothesis, he gaily replies that such is indeed the fact. And many years must elapse before this process is completed (p. 501). I tremble to think what the effect on my more conservative friends would be if they were told that the text of the Old Testament is to go into liquidation for an indefinite period, and will not obtain its discharge till it has been rewritten word by word. I suspect that Dahse goes beyond his real meaning in these *repartees*. Certainly "each verse" must be "accurately ascertained by textual criticism." But if Dahse imagines that it will be possible to obliterate all the subtle and pervasive characteristics which distinguish, say, the style of the Yahwist from that of the Priestly Code, he possesses a faith in the resources of textual criticism which is not "according to knowledge."

## NOTE XII—Page 164

"*Which seldom differ.*" To this Dahse retorts (Reply, p. 502) that in respect of the divine names (to which alone my words refer) the Sixtine and Cambridge Editions differ

in about thirty passages of Genesis; *i.e.*, in half as many cases as the Cambridge LXX differs at all from MT. The precise number, I believe, is twenty-two. But of these, four are cases where a name is wanting in one edition and not in the other; six differ only in the presence or absence of the article or a possessive pronoun; eight are cases where κύριος ὁ θεός stands in one text for κύριος or ὁ θεός in the other; only four present the absolute opposition of κύριος to ὁ θεός. My statement is not so wide of the mark after all.

## NOTE XIII—Page 166

### SUPERIORITY OF MT

Compare with the above Kittel, *Ueber die Notwendigkeit und Möglichkeit einer neuen Ausgabe der hebr. Bibel* (1901), p. 46:

"Es steht noch die Beantwortung der oben offen gelassenen Frage aus, welche der beiden Textgestalten als Ganzes den Vorzug verdienen möge und darum zu Grunde zu legen sei, die alexandrinische oder die massoretische. Bei allem Werte, den man auf die LXX als hervorragendster Hilfsmittel legen muss, kann doch kein Zweifel sein, dass die Tradition der Synagoge vollkommen im Rechte war, wenn sie jene Textgestalt, aus der der MT herausgewachsen ist, allen anderen ehedem umlaufenden Rezensionen der hebräischen Bibel, so auch derjenigen der alexandrinischen Uebersetzer, vorzog. . . . Es kommt dazu 'dass der hebräische Text als unmittelbarer Textzeuge immer einen Vorsprung vor dem indirekten Hülfemittel behalten wird,' sowie, 'dass keiner der alten Uebersetzer, höchstens mit Ausnahme der Targumisten . . . einen so klaren Einblick in den Textsinn gehabt und ihn bis in die feinsten Einzelheiten verstanden' hat, 'wie die traditionelle Lesung wie sie im massoretischen Punktationssystem vorliegt' (Buhl). Alle diese Thatsachen können uns nicht darüber im Unklaren lassen, dass die Richtung, in welcher wir uns beim Suchen nach dem besten erreichbaren Bibeltextes zu

bewegen haben, keine andere sein kann, als diejenige, in welcher der Massoretentext liegt."

## NOTE XIV—Page 176

In the note on Herrmann's observations on Ezekiel, Dahse (Reply, p. 491) seizes on the fact that *Adonai Yahwe* occurs 217 times and Yahwe alone 218 times; and inquires whether I consider this equality (?) fortuitous or a later juggling with figures. I reply that I take the former view; and ask in return if Dahse thinks that later jugglers could have brought about an artificial equality in strict observance of the rules pointed out by Herrmann. He then goes on to ask what I make of certain remarkable facts brought out by Hontheim in an article in the *Zeitschrift für katholische Theologie* (xxxiv. 625 ff.). I will answer that more fully. I happen to have examined Hontheim's conclusions when the article first appeared; and although I was at first greatly impressed by them, I saw reason to put them aside. The following table exhibits the *main* features of Hontheim's scheme sufficiently for my present purpose. He divides Genesis into nine sections thus:

| | | Elohim | Yahwe | | |
|---|---|---|---|---|---|
| I. | i. 1–iv. 26 ... ... | 40 | 30 = 70 | | |
| II. | v. 1–ix. 29 ... ... | 24 | 13 = 37 | | |
| III. | x. 1–xi. 26 ... ... | 0 | 7 = 7 | | |
| IV. | xi. 27–xvi. 16 ... | 0 | 29 = 29 | | |
| V. | xvii. 1–xx. 18 ... | 15 | 19 = 34 | = | 108 |
| VI. | xxi. 1–xxv. 18 ... | 18 | 27 = 45 | | |
| VII. | xxv. 19–xxviii. 22 ... | 6 | 18 = 24 | | |
| VIII. | xxix. 1–xxxvii. 1 ... | 32 | 10 = 42 | = | 108 |
| IX. | xxxvii. 2–l. 26 ... | 30 | 12 = 42 | | |
| | | 165 | 165 | | |

The two things that catch the eye here are (1) the equality in the total number of occurrences of E and J in

Genesis; and (2) the agreement in the sum of both names (108) in the history of Abraham (IV–VI) with that in the history of Jacob (VII–IX); as well as (42) in the two last divisions of the history of Jacob (VIII and IX). "Is this chance or intentional symmetry?" Well, as to (1) I observe that when we have two series of parallel documents (in chap. i.–xi. J and P; in xii.–l. J and P + E) of approximately equal extent, one using J and the other E, we may expect an *approximate* equality in the occurrences of these two names. But approximate equality is all that can here be made out. Hontheim's list of names is very correctly drawn up; but in order to produce exact equality he has to reckon the 20 instances of Yahwe-Elohim in chap. ii. and iii. as equivalent to Yahwe alone. That seems an unwarranted procedure: these ought surely to count both as J and as E; and if this is done the symmetry is destroyed. Moreover, while Hontheim excludes in principle the appellative uses of Elohim, he includes the following more or less doubtful cases: vi. 2, 4, xxiii. 6, xxviii. 17, 22, xxxii. 2, 3, 29, xxxv. 7. We cannot tell where the supposed authors of the scheme would have drawn the line, or if they would have drawn it at all; and it appears to me that, given an approximate equality to begin with, it would probably always be easy to make the correspondence exact by including more or fewer of such doubtful cases.—In regard to (2) it is apparent at a glance that the table as a whole exhibits great irregularity; and I am not prepared to believe that *two* coincidences out of so many possible manipulations are sufficient evidence of design. I believe, in short, that "jugglers with figures" could and would have gone much further. Hence my reply to Dahse's query is that I consider it highly probable that the coincidences to which he so vaguely refers *are* accidental. I have seen a hymn-board in a church where the number of the last hymn was exactly the sum of the other four; but it did not occur to me that the hymns had been selected with a view to bringing out that result. I will make this offer, however: when Dahse can show that similar relations obtain

in the remaining books of the Pentateuch, I shall acknowledge myself mistaken.

In view of the facts adduced in the second part of my note, I do not understand how Dahse can speak (p. 490) of the *regular* use of κύριος for *El*, *Eloah* and *Shaddai*, in the dialogues of Job. Such an assertion seems to me directly contrary to the facts. As for his further proofs, on the same page, of systematic alterations of the divine names in the LXX, and a preference of κύριος to ὁ θεός in certain later books, I have not examined the data he brings forward; and will only say that I am not after all greatly concerned with the habits of LXX translators, but with the practice of Hebrew scribes, and more particularly in the Pentateuch. That ὁ θεός came more readily to the pen of a Greek scribe than the Hebraic κύριος is, I think, true, even if certain Greek scribes had a partiality for κύριος.

## NOTE XV—Page 178

### THE TWO ACCOUNTS OF CREATION

With reference to this parallel, Dahse (Reply, p. 485) quotes Kittel's observation that in Genesis ii. Elohim seems to have been entirely or partly the prevailing name; and says that in that case it is impossible to speak of a "Yahwistic" account of the Creation as distinct from the "Elohistic" chap. i. As I have stated on p. 268, I am uncertain whether Kittel there means the original LXX, or the common original of both LXX and MT. On the former supposition (which seems the more probable), his subsequent admission that in Genesis ii.–x. the divine names have little analytic value, merely amounts to saying that a difference between the original LXX and the MT throws some degree of doubt on the soundness of both—which of course no one can deny (see p. 159 f. above). But if Kittel's remark applies to the original Hebrew text, then I owe my readers an explanation of how I can use the double name *Yahwe-Elohim* as an indication of a new document. It is just possible (though, from the general tenor of his criticism,

improbable) that Kittel has in view a critical theory, first propounded by Budde, according to which Genesis ii., iii. are mainly by a Yahwistic writer who avoided the name Yahwe *down to iv. 26*, where the worship of Yahwe is said to have been inaugurated by (or in the time of) Enosh. That would account for the use of Elohim (MT and LXX) immediately before in iv. 25; it would imply further that the rest of chap. iv. is the work of a different Yahwistic writer who never used any name but Yahwe; and lastly it would imply that in chap. ii., iii. the prevailing name was originally Elohim. How, then, it may naturally be asked, can the double name be treated as a sign of Yahwistic authorship? Here, of course, everything depends on the time and manner in which the Yahwe was prefixed to the (supposed) original Elohim. Obviously, *some* explanation of the insertion of the name must be found; and I have argued on p. 178 that Dahse's Pericope-hypothesis cannot explain it. The only satisfactory explanation in my opinion is that the double name is due to a revision of the narrative by a Yahwistic editor, who wished to carry back the name Yahwe to the beginnings of human history, but at the same time did not venture to remove the Elohim which he found in the text. If this theory be correct, and if we suppose the operation carried out *before* the amalgamation of the Yahwistic and Elohistic documents, it is evident that Yahwe-Elohim is the signature of the Yahwistic document, although originally only Elohim stood in the narrative. If, on the other hand, the Elohim is not original, then Yahwe must be so; and the application of the criterion is as simple as in all other cases. The only condition which would render the use of the divine name entirely nugatory as a criterion of source would be the assumption that, Elohim being the original name, the Yahwe was added at a late stage in the history of the text, after the composition of documents had been effected. But that, though of course possible, is on several grounds improbable. For a fuller exposition of the theory here outlined, see *ICC*, pp. 2 f., 53. On any view, be it remembered, the separateness of the two narratives is a fact.

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